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PSYCHIC RESEARCH IN THE ANIMAL FIELD: DER KLUGE HANS AND THE ELBERFELD HORSES

By Edmund C. Sanford

The story which I have to tell has to do in the first instance with the mental powers and behavior of horses, but in the end throws an interesting cross light on the mental and bodily behavior of man, and promises in its further development to illuminate some of the darkest corners of human psychology.

Ten or twelve years ago there appeared in a couple of German papers, one military and one civilian, a small advertisement offering for sale a seven-year-old stallion who could distinguish ten colors and knew the four rules of arithmetic. Practically nothing came of it; the advertisement was lost in the flood of general advertising. Again after an interval of more than a year there appeared in the same way a still smaller advertisement inviting readers of the papers to visit, free, certain experiments then in progress for determining the mental powers of horses. This time the arrow found a mark. The advertisement, by good luck, fell under the eyes of a retired Brigadier General of the German army, Zobel by name, an acknowledged expert in matters pertaining to horses. General Zobel visited the address given, saw what the horse could do, and in course of time published in the supplement of one of the Berlin daily papers an account of what he had seen, thus bringing *der kluge Hans* and his master for the first time to public notice. The general's name and fame caught public attention and sent others to see the wonderful horse for themselves and they in turn interested still others, until the thing became a veritable midsummer sensation. Discussion followed in the newspapers of Berlin and other German cities and echoes of it penetrated into Italy and America.

There were not a few skeptics, of course, who thought the thing was a fraud, that the horse was merely manipulated in some clever way by his master. But there were circumstances which spoke against this. It was hard to find any reason for the fraud and though some men like to gull their neighbors, they do not do it for long together without some purpose in doing it, and here a motive seemed lacking. There was no money feature connected with the exhibitions and no attempt of the master to exploit himself or the horse. He refused altogether to exhibit upon the vaudeville stage. And still more to the point—the horse solved problems for others beside his master, some of them men whose good faith could not be called in question. And finally a group of well-known gentlemen, including army officers, experts in horse matters and animal training, teachers, men of affairs and scientists, who had gathered as a voluntary investigating committee, studied the performances carefully and reported unanimously that they had been unable to discover anything whatever in the nature of fraud, that no intentional signals were used, and that so far as they had been able to discover there were not even any unintentional signals of any known sort. This last the general public took to be equivalent to an assertion of the committee's belief that the horse actually did on his own account the things that he was reported to do. This of course served only to whet public interest still further. Conjectural explanations were many but nothing like a final solution appeared, until a small group of experts took the matter in hand, and after a couple of months brought forward an explanation which was generally accepted as the final verdict of science and brought discussion to an end.

This is the outline of the first chapter of our story; let us come to the details of it. Let us imagine that we have obtained an invitation to one of the exhibitions, for they are semi-private, and are to see the thing for ourselves like the other Berliners. We take our way to the northern portion of the city and reach finally, in a rather poor district, a paved court, none too clean, surrounded by high tenement houses. In this court often before very distinguished company—besides uninvited guests in the tenement house windows and boys on the roofs and fences—the remarkable things take place.

The horse is a black stallion of Orloff trotting stock about eight years old and his master is Herr von Osten, a retired Prussian schoolmaster, now past sixty years of age, and dressed usually in a long, seedy overcoat, or "duster," and slouch hat. There are no restrictions as to the place we may occupy. We may come as near as we choose and look on from any side. The horse is entirely free and is managed wholly by means of the voice with frequent rewards in the way of bread and carrots, for which he is eager, as he gets little exercise and is therefore on short rations. He is spoken to in German, but without special emphasis. If he understands a question, he signifies it, we are told, by nodding his head; if he does not understand, he moves his head from side to side. By head movements also he indicates directions: up, down, right, left. We are warned that he will not reply to questions addressed to him in French or Latin and that he does not understand all German words, though he appears to comprehend a great variety of questions, or at any rate answers correctly, and his vocabulary seems to increase daily.

His powers of communication are, however, decidedly limited; he moves his head, he can go here and there and do this or that, and finally he can indicate numbers by tapping or pawing with his fore foot; and in this way the answers to questions are generally given, his master having devised special means by which answers can be indicated by appropriate numbers.

When the exhibition begins, this is the sort of thing that we witness. His master, standing near him, addresses him in a kindly way and shows him an arrow, a circle and a square drawn on three little blackboards hung up conveniently and asks him to pick out the arrow. Hans goes to the proper board. In the same way he tells which is the square and which the circle. Herr von Osten then asks, "How many corners has a circle?" to which Hans very properly replies by moving his head from side to side; there are no corners. Next several gentlemen are placed in a row and Hans indicates correctly, by tapping off the proper place number, which is the tallest, which the stoutest, which are officers, which have swords, which has his arm in a sling.

These tests, which can be made in very great variety, seem to show that Hans can understand what is said to him; but the test with the row of gentlemen has really involved more than the mere comprehension of words; the horse has counted, and counting, if carried, say, beyond three, involves something like abstract thought. He will, moreover, count anything you like, or tap off any reasonable number, and seems to show intelligence in it too; for he executes small numbers slowly and large numbers quickly, raising his foot but little at first as though he had a long lot of tapping to do and wished to get through the unessential parts as quickly as possible. After giving the last tap he brings his foot back to its original position or sometimes finishes with a smart stamp with his left foot. For zero he moves his head from side to side.

Counting is an abstract process, to be sure, but a simple one, and his master goes on to ask other questions: How much is 4 times 4? Hans replies with the required 16 taps. How much is 4 and 4? Hans gives the necessary 8. In the same way he gives the answers to problems in division and subtraction. He is asked, for example, By what numbers is 28 divisible? And we get in series 2, 4, 7, 14, and 28 taps. Fractions are no bugbear to him. He will change common fractions into decimals or vice versa or work with either. "How much is 2/5 and 1/2?" his master asks, and Hans taps off 9 for the numerator and 10 for the denominator: 9/10. He usually works with numbers below 100, and when he deals with larger numbers, as is sometimes required, he is said to handle them by analogy with what he knows of the lower ones, showing in this way, if the statement is true, a very good grasp of the abstract plan of the number system.

When, as sometimes happens, he has given a wrong answer, it is usually only necessary to ask him "By how many have you miscounted?" to receive the appropriate correction. Sometimes, however, he seems cranky and having given a wrong answer will stick to it, and sometimes he seems only to want to joke with his questioner.

Now his master turns to reading and spelling. Certain words are written on the little blackboards—common words or the surnames of persons present,—and Hans will, when one or another is pronounced, indicate the serial number of the board on which it is written, or, if required, go to it and touch it with his nose. In a similar way when asked for the color of certain bits of colored cloth, he indicates the number of the board bearing the name of the color or goes to and touches the bit of cloth having the color named. He will even match colors on occasion or correct a false indication when his attention is called to it.

He spells by means of a chart on which the letters are written in tabular form so that each can be indicated by the numbers of its row and column. Thus in reply to the question: "What has the lady in her hand?" he taps off the series 5-2, 3-2, 4-6, 3-7, which being interpreted spells *Schirm,* the German for sunshade, *sch* being treated in the tables as a single letter. He seems to remember well what he has spelled out in this way and will also spell out the surname of persons who have been introduced to him.

Now the master shows him a watch and asks him what time it is, and being told follows with questions like these: "Between what figures will the small hand be when the time is five minutes after half past seven?" or "Over how many minutes must the large hand move to get to the position of a quarter of the hour, when it starts at 7 minutes after a quarter past the hour?" to all of which he gets correct replies. But in other respects also Hans seems at times to show keen powers of observation. When asked, for example, how many fingers a certain gentleman of the company had on his right hand, he promptly and to the surprise of the spectators answered *four,* which examination proved to be correct.

And now his master tries him at indicating the value of German coins, the signification of playing cards, at giving the days of the week and of the month, the emperor's birthday, and the identification of people by their photographs, in all of which he succeeds as usual.

There is not much evidence that horses are very discriminating musically—rather the reverse—but Herr von Osten now proceeds to demonstrate in Hans a definite knowledge of music. He blows a tone upon a sort of small keyed harmonica, and asks Hans to tell the tone given, which he does by means of a chart upon which the notes are written in such a way that each can be indicated by a number. The master sounds a discord (*c, d* and *e*) and asks, "Does that sound well?" Hans moves his head from side to side. "What tone must be left out?" Hans stamps twice for the *d*. He answers correctly also when asked to name the various musical intervals used, both when the tones are sounded simultaneously and in succession—feats which in a human subject would deserve the name of fair musical talent.

Such things as these were not done of course in continuous succession, perhaps not all on the same day, and always with short intervals of rest and frequent rewards of something to eat. Nor did the exhibitions always run smoothly; Hans sometimes made mistakes and was sometimes intractable even in the hands of von Osten. Nevertheless in course of time things like those I have mentioned were done again and again in the presence of all sorts of spectators and with all sorts of variations. They were done, in the absence of von Osten, by several gentlemen who were interested in the case, though usually with a greater proportion of failures, which might easily be due, it seemed, to unwillingness on the part of the horse or to unfamiliarity with a strange questioner.

One very remarkable, though perfectly authentic instance of Hans' performances, I have reserved until the last as it seems to show even super-normal powers. The following experiment was made a number of times by Herr Schillings, an African explorer and a man of scientific standing, who very early succeeded in getting Hans to answer questions for him. While Hans was standing in his stall, Schillings would approach, accompanied by a second gentleman. At Schillings' request this gentleman would think intently of a number between 1 and 20, but would keep it absolutely to himself. Schillings would ask Hans to tap off the number thought of, and then would withdraw as completely as possible from the performance, in other words, would remain absolutely passive. On several occasions Hans played his part as mind-reader with astounding success, tapping off correctly the number unspoken and absolutely unknown to Schillings.

Here then is our problem—how to account for the doings of a horse that to all appearances understands spoken and written words, knows the calendar and can tell time, can do the abstract thinking required in arithmetical work, and has besides very fair powers in music and other directions—who on *prima facie* evidence lacks, as his admirers asserted, nothing at all of the human mental outfit except the power of speech.

Of solutions there was no lack. The explanation of his master, von Osten, was the very simple one that the horse had the mental powers which he seemed to have—that he was able to think and as a matter of fact did think for himself and answer the questions put to him. Hans had a mind, and his mind was capable of education. What Hans was now able to do was the natural result of a thorough and careful course of education. He (von Osten) had spent four years of hard and patient labor in *educating* him, making use of the same pedagogical principles that are in use in the teaching of children. Compare an untaught child with a well-taught one. The difference is great, and such a difference is as natural in horses as in children. The result seems astonishing no doubt, but it is so simply because nobody has ever before made a serious attempt to *educate* a horse. Nothing, in fact, irritated the old man more than the suggestion that Hans was merely a trained animal and not an educated one.

In this opinion, at least as far as that the horse could think for himself, von Osten was by no means alone and many of his supporters were men of distinction. Even Prof. Stumpf of the University of Berlin (who served on the original investigating committee and under whose general oversight the later studies were made) was beginning, as he himself confesses, to set his mental house in order for the acceptance of the notion that horses have some educable rudiments of abstract thought—provided such a conception should at last seem unavoidable.

Over against this explanation, however, stood the diametrically opposite view—the opinion that Hans was trained, and not educated; that his so-called answers were merely pieces of behavior associated by repetition with certain objective conditions which served as cues. When he names the tones which have been sounded in a given chord, he sees the keys of the instrument move and moves his foot as many times as he has been trained to move it under such circumstances, knowing nothing either of music or of counting. His numerical feats are not calculations at all, but mere matters of memory, suiting a certain amount of tapping to a certain sound of words. This demands an enormous power of general retention; but then, it was argued, the memory of the horse species has lain fallow for untold ages and we may therefore expect a phenomenal crop from it. This view of course depends upon strange notions of the nature of mind and is a great strain upon one's credulity, but nevertheless it had its supporters.

A third party would hear of neither a thinking horse nor of one with a long memory—though they were ready enough to accept the idea of signals of some sort. Indeed they rested their whole explanation on signals. Most of those who held this view had no hesitation in declaring that the horse was controlled by intentional signs given by von Osten—that the whole affair was nothing but clever circus trickery. And in this group were found in strange alliance those to whom for ulterior reasons (philosophical, theological or otherwise) the idea of a thinking horse was repugnant, those who rejected a thinking horse simply as opposed to common sense; and, lastly, most of the professional horse trainers who felt that they knew the business of trained animals so perfectly that they might save themselves the trouble of examining this case, or who were satisfied with a very cursory examination.

A few of those who held to the explanation by signals thought the signals might possibly be unintentional, but they did not venture to specify in detail what sort of signals were in use, nor, with one possible exception, offer proof for their hypotheses.[1] Those, on the contrary, who believed in intentional signaling suggested a great many; the signals were somehow given by means of Herr von Osten's old gray slouch hat or by his long overcoat or with his hand as he reached into his pocket for carrots. Others said the signs were eye movements or movements of the hands or were given by changed intonation in asking the questions. But many observers, including the only experienced horse-expert who studied the case carefully, had failed to find that signs were used.

And then finally there were those who could not satisfy themselves at all with explanations of the ordinary sort and sought out extraordinary ones. Some suggested that the horse responded to radiant heat streaming out from the questioner, or to electric apparatus in the ground acting on the horse's hoof, or *n* rays emanating perhaps from the brain of Herr von Osten, or waves of thought proceeding from the same source, or magnetic or hypnotic influences, or last of all —and capable of everything—"suggestion!"

Taking all these together and neglecting small differences we have four rival hypotheses: 1) The horse can think for himself; 2) The horse cannot think, but has a phenomenal memory and is a marvel of training; 3) The horse has no need to think or even to remember, but is manipulated wholly by signals given by his master at the moment; and 4) The affair is occult and to be explained by the operation of powers and influences of which we know less than we do of horses.

With such a wealth of hypotheses it is clear that what is most needed is a careful sifting out of the facts, and, if possible, the testing of what seem to be facts by experiments carefully planned and methodically carried out. And these were fortunately forthcoming. A thorough study of the horse and his performances was begun by Prof. Stumpf and his assistants—one of whom, Dr. Oskar Pfungst, gave himself especially to it, and was able before long to let in a flood of light upon the mystery.[2]

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[1] The exception was Cav. Emilio Rendich, an Italian painter resident in Berlin, whose part in this matter will be referred to later.

Scientific method is simply systemized common sense, and the first step was, naturally, to find out whether or not the horse could think for himself by testing him with questions the answers to which were unknown to the questioner, or any one else present. Some tests of this sort had been made by the original investigating committee and had seemed to show that under such circumstances Hans could answer correctly. It was therefore essential first of all to put this matter to the most rigid possible test and confirm or demolish these supposed results. And to do so it was necessary to arrange the tests in such a way that the chances of purely accidental successes and failures might be estimated, in other words, to alternate questions in which the answer was known to the questioner with those in which it was not known. If under these circumstances Hans succeeded as well in one case as in the other, his ability to think would be fairly demonstrated; if he failed in the trials in which the questioner did not know the answers and succeeded in those in which the questioner did know the answers, his dependence upon the questioner would be as fairly demonstrated, and the discovery of the nature of the dependence would be the next problem to be undertaken.

Let us see what the results of such tests were like. The experiments were made in a tent set up in the courtyard where the exhibitions had taken place. A tent was used in order to shut out distractions and cut off the possibility of long distance signals. The questioner was usually, though not always, Dr. Pfungst, who had worked enough with Hans to have shown himself entirely competent. At times several persons were present, including Herr von Osten, but at other times also Dr. Pfungst worked alone.

Let us see whether Hans can read numbers. A set of figures printed or written on cards is shown to him one by one in such a way that he alone can see them, and he is directed to tap off the number shown. In approximately alternate cases the conditions are the same except that the number is known to those present, including the questioner. This test was tried in all 91 times (49 when the number was unknown to the questioner and 42 times when it was known). Hans was right 4 times in 49 when the number was unknown to the persons present; and he was wrong in only one case in 42 when the number was known to them. His four successes —less than one in ten—can hardly go down as more than lucky hits. And we may add that the result was not essentially different when von Osten himself acted as questioner instead of Pfungst. Such results throw Hans' ability to read figures on his own account into very serious doubt.

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[2] The first authentic account of Pfungst's work was a brief report by Prof. Stumpf in December, 1904. Pfungst's own complete account appeared in German in 1907 and in English translation in 1911. The present account of the Hans case was drawn chiefly from Pfungst's original work.

Similar experiments with words, written on the little blackboards and hung up in a row, which Hans was to "read" by tapping off the number of the board carrying the word called for, succeeded no better, though the words were such as Hans might be supposed to know, such words as *Hans, stall,* and the names of the colors. Out of 26 trials in 14 of which the words were known to the questioner and in 12 unknown, Hans never failed to tap off the right number in the first case and never succeeded in hitting the right number in the second case. His reading of words would seem to be of the same doubtful nature as his ability to read figures.

Let us see how he does with arithmetic; perhaps he can reckon better than he can read. A number is whispered to him by Herr von Osten and a second by Dr. Pfungst; Hans is to give the sum. This was tried 62 times, 31 in each condition as regards knowledge, or the lack of it, on the part of the questioner. When the questioner knew, Hans was right 29 times; when he did not know, Hans was right 3 times—again in less than 10% of the trials.

The story is the same when he was required to spell or to count: when the questioner knew, Hans was right; when the questioner did not know, Hans was all at sea. The case looks dubious for his ability to deal with numbers and for the assumption of powers of abstract thought based upon that ability. But let us try his memory and his stock of information. When asked to give the day of the week on which certain days of the month would fall when Sunday was the first, he failed in 10 trials out of 14, but succeeded in 4. This is a better record than any other so far, but even this we cannot allow to stand to his credit, for the answers to just these four questions were known to the hostler who was present during the experiments. Under similar careful experimentation his musical knowledge—both his recognition of the pitch of tones and their relation to one another—melted away. If the questioner knew, Hans knew; if the questioner did not know, Hans did not know.

Taking the tests all together, the evidence seems unmistakable. Hans does not think for himself, but has some way of drawing upon the information of his questioner or others in his presence. He clearly does not pass his examinations on the honor system.

The less systematic tests of the original investigating committee and those of others who at various times appeared to get different results, but with reference to whose care in observing the proper precautions there is doubt, cannot be given weight in the face of the uniformly contrary results here attained. It is easier to believe that in those cases some essential precaution was neglected than that any peculiarity in the procedure of Dr. Pfungst should have brought such regularly negative results as he and his colleagues found.

Our first question then—whether Hans thinks for himself in a human way—may be answered in the negative with practical certainty: He does not. But in answering it in this way we have rendered the case, if anything, more mysterious than before. Hans is dependent in some way on the information possessed by his questioner, but: How? We are face to face with our second question.

Does he get his information or his cue through some one of his five senses or in some less usual and perhaps occult way? Is it a matter of seeing, hearing, touching, tasting, smelling, or of thought transference? Naturally we try the senses first and begin to test him again when he is for the time prevented from using one or another of them; and, first of all, sight. Hans is therefore rigged out with large blinders which are so arranged as not to bother him at all in themselves but effectually to cut off his vision backward. The questioner now gives Hans numbers to tap off—first standing back where he cannot be seen and then again asking for the same number while standing forward where he can be seen.

I said the blinders were so arranged as not to bother the horse at all "in themselves," but the experimental conditions as a whole evidently bothered him a good deal. He always tried hard to get sight of the questioner when the latter stood out of sight, backing up to do so, and when an attempt was made to tie him so as to prevent this, getting fractious and breaking his rope, though usually he made no objection to being tied. As a result of his efforts to get sight of the questioner, it was not possible to be sure in all cases whether the test was made with or without sight of the questioner. In 35 out of 102 trials he certainly did not see the questioner; he certainly did see him in 56 cases; and in 11 it was uncertain.

In the 35 cases without sight of the questioner Hans was successful but twice; in the 56 cases with sight of the questioner he was right 50 times; and in the 11 doubtful cases he was right again twice. The result is clear: when he saw, he succeeded; when he could not see, he failed. To get this result it was necessary, however, to look carefully to the size of the blinders and to their putting on. Experiments with less care in these respects gave a much larger proportion of successes.

Incidental observations also seemed confirmatory of the importance of vision. The horse was more apt, so far as one could tell, to look at his questioner than at the persons or things he was supposed to be counting or at the words he was supposed to be reading; and he was apt to get uncertain in his answers if experiments were continued into the twilight.

From these experiments and observations it seemed clear to Dr. Pfungst that Hans got his information through his eyes; but did he get it all that way; did he get any through hearing or his other senses? It seemed rather unlikely that he depended upon other senses than his eyes. As regards hearing it was discovered that he could answer correctly when the questioner made no sound whatever—merely *thought* the question and *thought* the answer; and further that the animal was not usually disturbed in his tapping by loud calls or commands provided they were unaccompanied by movements; and, finally, it was noticed that he rarely or never cocked his ears in the direction of the questioner, a thing that horses are very prone to do when listening for any particular sound.

At any rate no further tests of the senses were made by these investigators; and here, perhaps, in view of later developments, lies a serious incompleteness in their work. It was very natural, however, that they should not follow sense tests further at this time; for at about this stage of the work Dr. Pfungst was fortunate enough to discover in the case of Herr von Osten himself certain very slight, almost imperceptible movements of the head and body—movements often as small or smaller than one one-hundredth of an inch in extent, which seemed clearly to be the visible cues by which the horse was controlled. Later he traced such movements in others for whom Hans would answer questions, including himself, was able to point them out to other observers who could then see them without great difficulty, knowing what to look for, and finally to make them voluntarily and thus to draw from Hans any answer to any question, or no question at all.

When the questioner addressed a question to the horse he bent forward very slightly to see better the movement of the horse's foot. This forward inclination served as a starting signal and Hans began to tap. When the required number was reached, if the questioner knew it, he involuntarily raised his head ever so little again (as one might do when the need for the closest attention was past) and this served as a signal for the tapping to cease. Then the trial being over the questioner usually stood erect again, though this larger movement did not serve at all as a signal, except in a negative way. In case it did not follow promptly on the slight raising of the head (the true signal for cessation) a single additional tap with the left foot followed, after which, if one still remained bent, tapping with the right foot would be resumed. One might stand before Hans and ply him with questions as long as one pleased, but never an answer was forthcoming as long as one stood perfectly still and erect. The tapping began on the forward bending of the questioner and the tapping ceased when the slight upward movement of the head was given, and did not stop without it, though it appeared later that other somewhat similar movements of other members than the head might also serve. The rate of tapping was also found to vary with the amount of stooping on the part of the questioner. If the latter bent far forward Hans worked rapidly; if only a little, then more slowly; and there were many other confirmatory details.

Usually, as it seemed, the horse was attentive to the questioner alone, and stopped and started on signals from him only; but in the case of the days of the month he seems to have stopped at signals from the hostler, and the cases of apparent mind-reading above mentioned were undoubtedly of a similar sort. Herr Schillings started him tapping, the gentleman whose mind was read unwittingly stopped him. There was nothing else necessary.

Minute movements of a somewhat similar character are a common means of control in the management of trick horses. The point here is that the movements were extremely minute and that there seemed no reason to suppose they had been adopted consciously by those for whom the horse responded.[3]

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[3] It would appear that the honor of the first observation of these minute unconscious movements of von Osten and others does not belong to Pfungst, though there is no question that the full demonstration of their importance is his. Cav. Emilio Rendich, already mentioned, after a period of belief in the independence of Hans' performances was later thrown into doubt on account of their in-

I have gone into some detail with these cases in which Hans answered by tapping because they are the most interesting and were the most fully studied, but it will be recalled that he answered certain questions by head movements—assent and negation by nodding and moving the head from side to side, and the directions up, down, right and left by corresponding movements. These, like the tapping movements, proved to be given in response to minute but appropriate head movements on the part of the questioner. Other answers were given by going to specified objects, to the colored cloths or the little blackboards, for example, and fetching them or touching them with his nose. In this he was guided by the position of the questioner who usually faced toward and looked directly at the object in question. In experiments of this kind Hans was not uniformly successful at any time, even when helped out by word of mouth. When the questioner called out to him "Wrong," or "Look out," or repeated the color name, he would give up the thing he was about to take and take another, but not by any means always the right one; and in this way—by calling out—his answers could be quite variously manipulated. In a word there was nothing in the experiments on these matters to invalidate the results of the studies of the tapping, and we may sum the whole thing up by saying that they seemed to Dr. Pfungst and his colleagues to put it beyond question that all of the answers which Hans gave were merely sets of movements controlled by signals from his questioner.

And so we have, as the result of these careful studies, a horse proved to be destitute of the human mental powers with which he had been credited, but demonstrated to be astonishingly responsive to almost imperceptible movements, and ready enough to make use of them and of the faculties of such a mind as he had for the sorts of results in which he as a horse was interested, namely, bread and carrots.

It may help us to understand his cleverness in catching these movements to recall that movements as such are seen by ourselves with some ease even in the remote parts of the visual field where we can see little else; that the attention of animals

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creasingly extraordinary character and set out to find some other explanation. His search was rewarded by the discovery in von Osten and Schillings of slight movements which seemed to guide the horse in his tapping. Later Rendich trained his shepherd dog, Nora, to respond to similar movements and exhibited her to several members of the original Hans committee. His discovery antedates the work of Pfungst and his associates, and Pfungst is said to have seen the dog work, though it is not, of course, by any means certain that Rendich at the time explained his methods.

in general appears also to be caught very easily by movements; that there seems some reason to believe that the eyes of horses are unusually well adapted anatomically for the perception of movements; that the detection of small movements of their enemies would be of the greatest advantage to horses in their wild state; and that Hans himself had had a four years' course in perceiving those of Herr von Osten.

So much for the animal side of this remarkable case. The human side is no less important; how does that stand? Were the signals really unconscious in the case of Herr von Osten? Is it possible that he was absolutely ignorant of the movements by which for years he had been managing the horse? Or was he after all but an unusually clever trickster with a more than usual appetite for humbug. This last inference is one that is easy to draw and was drawn by many. What is the evidence pro and con?

In his favor are, first, the strong impression of sincerity which he gave, and the absence of a motive, which we have already mentioned; and, second, the judgment of the first investigating committee which had this point especially before it and which exonerated him of fraudulent intent. This by itself is not wholly convincing, perhaps, but it gathers force if we can show, on the one hand, that such movements may exist in full unconsciousness, in fact do so exist in the case of many people, and further that the course of training through which von Osten had put Hans was such as to offer opportunity for the cultivation of such movements and their unintentional growth into a system of signals such as was found to exist.

On the first point we have experimental evidence from Dr. Pfungst. As a portion of his study of the case Pfungst repeated in the psychological laboratory, as nearly as he could duplicate them, the experiments made with the horse in the court-yard. He himself took the rôle of Hans, tapping with his hand and watching for signals from the person who acted as questioner. He tried in this way some 25 people of different age, sex, and nationality, and he found but two persons in all who did not make unconscious movements of some sort by which he could tell when he had reached the numbers of which they were thinking intently, and one at least of these stolid ones was also wholly unable to get answers from Hans. Pfungst could even tell in some cases how simple problems in addition had been formulated,—whether, for example, the questioner had thought of $5+5+2$ or $2+5+5$. And his mistakes, which were not infrequent at first, were of the very same character as those made by Hans. Similar unconscious movements were observed when directions were thought of—up, down, right, and left—and when particular objects were designated after the manner of Hans's row of colored cloths. It has been shown also by others (Lehmann and Hansen) that many persons in thinking of numbers whisper them unconsciously but in a way that can be heard at a distance by a proper adjustment of sound reflectors.

Unconscious movements are in all probability extremely common—the normal and regular thing, in fact—especially under conditions of considerable concentration, when (from the very fact of concentration on something else) their unconsciousness is doubly guaranteed. There is therefore no reason for thinking it strange that von Osten should make unconscious movements or that he was a fraud because he did not admit it but was caught in the act by Pfungst and others who watched him. He simply did not know the movements existed; and Pfungst here comes to his aid again with the observation that he himself found the movements easier to detect objectively in others than subjectively in himself, although he knew, of course, at the time that he was making such movements.

That such unconscious movements once present could grow up into a habit on the part of the master and into a set of signals for the horse is not hard to understand when we think of the process of training through which the horse was put. The old man stooped over anxiously to see that the horse tapped as he should—indeed, in the earlier stages of the training, the master with his hand assisted the horse in executing the proper number of taps—and then straightened up the least little bit, taking an attitude of a little less effort, when the required number had been reached. This sort of thing had gone on hour after hour and day after day for something like four years. There is room here and to spare for the forming of unconscious habits on one side and the perfecting of training on the other.

After the publication in the end of 1904 of an abstract of Pfungst's work with its annihilating assault on von Osten's conception of Hans' mentality the case seemed closed. The newspapers commented briefly on its outcome, and the general public turned to other matters, while the old man forbade further study of the horse by outsiders and doggedly took up again his patient teaching of him, working him out, as he believed, of the habit of responding to signals, into which the scientific experimenters had stupidly trained him. Thus ends the first chapter of the story.

The second chapter opens with the entrance of a new figure, Herr Karl Krall, a wealthy jeweler of Elberfeld. Krall had followed the Hans discussion in the newspapers and was one of the few who had remained unconvinced by the results of the Pfungst studies. About the middle of 1905 he visited von Osten and gradually prevailed upon the dour and crotchety old man to permit and even to co-operate in new experiments. These were continued at intervals up to 1908 and confirmed Krall's belief that Pfungst was wrong and von Osten right—that the horse actually did his own thinking. Later Krall purchased other horses for experiments on his own account, especially two Arabian stallions whose attainments under his teaching soon far surpassed those of Hans at his best. And finally he published last year a handsomely illustrated volume summarizing his work both with Hans and his own horses and the inferences which were to be drawn from the results which he obtained, and thus reopened the whole controversy.[4]

Let us see first what Krall got from his experiments with Hans. Early in his studies he undertook a repetition of the experiments made by Pfungst to determine whether the horse was able to answer questions when he could not see his questioner—the experiment with the blinders. Pfungst appears to have worked with large blinders on both sides of the horse's head. Krall worked with a single large blinder on one side, arranged, as he assures us, so as to correspond " in every way to the most rigid critical requirements." In the picture given to illustrate this outfit the blinder is re-enforced by a large piece of opaque cloth which covers the rear half of the blinder and most of the horse's head. In the execution of the experiments the questioner and his assistants stood at a distance of six or eight meters from the horse on the blindered side, to meet Pfungst's finding that at distances less than four and a half meters the control of the questioner when visible was still in some degree effective.

After a time sufficient, as Krall thought, to accustom the horse to the unfamiliar and bothersome limiting of his usual field of vision and to the new conditions Hans worked as well with the blinder on and with the questioner at a distance as in the more usual conditions. It was the same when experiments were made with the blinders on and at night. The horse apparently could work without sight of questioner.[5]

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[4] Krall: Denkende Tiere, Leipzig, 1912, pp. 532.

[5] We must note, however, that in Pfungst's experiments the alleged unfamiliarity of the blinders did not distract Hans to such an extent as to hinder his attention to Pfungst or his answering nearly

These were by no means the only matters upon which Krall tested Hans in the five months or more scattered through several years during which he worked with him. He instituted regular tests of his keenness of vision, his discrimination of colors and forms, his liability to optical illusions, his senses of hearing, smell, taste, and touch, his speed of comprehension and his span of attention,—carrying out all these tests in the general manner in which they are made on human subjects. He even experimented upon Hans' powers of appreciation of the beautiful and ugly, and his consciousness of self. And through it all he had not infrequent, though undesired, occasion for observing the horse's willfulness, stubbornness and contrariness, features of horse psychology upon which in his later discussions he lays no small stress. Into all this, however, we have no time now to enter; nor do these experiments and observations, except perhaps those with reference to the horse's crankiness, touch the main question. If the horse has the independent mental endowment which von Osten and Krall believe he has, it is not surprising that such tests should have been possible; if the horse is guided, visually or otherwise, by some human being within sensory range, as Pfungst and others believe, there is no reason for surprise at any results within the limits of human capacity. What still remain the chief questions are the two upon which Pfungst worked: Can the horse think for himself, and if not, by what means does he draw upon his human environment? It is these two questions also that we shall keep uppermost in mind in our account of Krall's work with his own horses, Muhamed and Zarif.

But before we take up the training and feats of these horses, let us turn for a moment to their master. What manner of man is Krall? To this question we reply, depending here chiefly upon the statements of his partizan, Dr. William Mackenzie, that Krall is a respected member of a commercial family of wealth and importance in Elberfeld, still in busi-

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all Pfungst's questions correctly when Hans could see him, and that in Krall's experiments in the dark there was some of the time light enough to distinguish objects as small as unlighted matches and to make it interesting for spectators in the tenement windows about, that in other experiments a lighted candle was used in the experiments themselves and in still others small light disks or transparencies—sources of light of possible consequence in case of an animal with as good night-vision as a horse. Under the circumstances the question naturally suggests itself whether the time supposed by Krall to have been necessary for accustoming the horse to the unfamiliar blinders was not in reality necessary for the establishment of a new sort of unconscious sensory guidance in place of that cut off by them.

ness and following science as an avocation. He is regarded by his neighbors as perhaps a trifle visionary, but of unquestioned good faith. Temperamentally he is open, enthusiastic, liable to say things of which his opponents can take advantage; he is inventive, full of the spirit of research, of a philosophic habit of mind, with something like a touch of genius. Before he became interested in Hans he had long devoted himself to psychological experimentation, but without publishing his results. In his house he has a considerable physical laboratory. His library is that of a student and especially strong in psychology.

His general standpoint in animal psychology is indicated by his contention that animal and human minds are of identical nature and that neither has come into being through evolution. The great bar to the recognition of this identity has been the absence of adequate means of communication between men and animals. It is von Osten's glory that he discovered a way by which this bar could be removed. Krall's own function has been to save von Osten's great discovery from oblivion (impending on account of the blunders of Pfungst and the bigotry of science), and to set it upon a firm basis of demonstration, paving the way to a complete mutual understanding between men and animals, with infinite possibilities of advantage to the animals because the concession of likeness in mentality necessarily involves also the concession of personality and a new set of ethical obligations on the part of man toward his fellow creatures. Krall mentions incidentally his affectionate interest in animals from his youth and his gratification that destiny has assigned to him the demonstration of their possession of reason and the general unity of Nature in the things of the mind. This attitude toward his own work and that of von Osten goes far to explain certain characteristics of his work in general and of the book which he has written.

In beginning the training of his own horses Krall introduced several minor improvements in method. He provided a special inclined platform on which the horse might work more easily and tap off his replies more distinctly; he altered the system of tapping, so that units were tapped with the right foot, tens with the left, and hundreds again with the right, thus improving the tapped replies both in speed and accuracy. When it came to reading and spelling he introduced a new and better letter table. He carried out the training also in such a way as to make the work as little fatiguing to his pupils as possible. He did not, on the other hand, dispense wholly with the whip as von Osten had done.

For these or for other reasons the horses made phenomenal progress. After two weeks' training they executed simple commands and Muhamed had begun to show his arithmetical ability; he knew the numbers to ten and had caught the ideas of addition, subtraction and multiplication. During the first month the horses learned to perform commands given in German and in foreign tongues, both spoken and written, including Greek. After five months of training they had reached the mental level at which Hans had arrived at the end of four years. They surpassed him even then in the independence of their expression, and as their training continued soon surpassed him in all particulars.[6]

Of the attainments of Muhamed and Zarif I can give but a few samples, and I select, of course, striking instances, omitting the multitude of similar but less remarkable cases; I shall also confine myself to the results with arithmetical problems and in the use of language, neglecting many other lines of education from which hardly less astonishing results are reported,—as, for example, in the miscellaneous notes (pp.

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[6] An interesting item as to the manner of training and one that is not without possible significance for the method of the later work of the horses comes to us from the Düsseldorf veterinarian, Dr. Karl Wigge, from whom the following observation is cited by Ettlinger (*Der Streit um die rechnenden Pferde*, pp. 45 ff):

"Wigge was able in the case of Berto [a blind stallion], who was led by the bridle, to assure himself that the blind stallion 'regularly began to tap at once with the right foot when Albert, the groom, after Krall had put the question, let loose the bridle, which he had before held taut.' In the same way Wigge reports with reference to another horse, Demir (able to see but blindered), that in his case also Albert held the bridle and gave signals with it. The question was the simple arithmetical problem 3 + 3. 'So soon as the horse had tapped with the right foot up to 6, . . . Albert gave the bridle a little jerk and the tapping promptly ended. In case of numbers above ten, which have now passed from my mind, the jerk for the units was given with the right rein, for the tens with the left. Of especial interest in this connection was the question: How does one make zero? Albert pinched the horse in the right flank (turning of the horse's head to the right), then in the left flank (turning of the head to the left) and the zero was finished. No one of the spectators, so far as I could judge, had seen anything in this process, and Krall himself neither at that time nor in his book has given the least intimation of anything of the sort. The spectators were promptly charmed with the success of the pupil and clapped in lively approbation.'" (Wigge: *Deutsche tierärztliche Wochenschrift*, Dec 7, 1912.) The use of these means of control is not denied by Kralls partizans, but they explain that such means are used only in the early stages of the training and later become unnecessary.

490 ff. of Krall's book) which recount cases in which Muhamed dictated problems, drew inferences as to the identity of snow and rain, guessed simple riddles, recognized verbs, found rhymes for words given, and distinguished the concepts of beauty and ugliness.

Most of our instances will also be taken from the doings of this same Muhamed who is the brightest of Krall's stud and especially strong in the extraction of roots. Krall himself reports the solution of such examples as the following, the examples being written on the blackboard (pp. 119 f.):

$\sqrt{64} \times \sqrt[3]{64}$ first answered wrong as 42; then right as 32.

$\dfrac{\sqrt{36} \times \sqrt{64}}{50-2}$ answered with emphasis as 1.

Visitors writing since the publication of Krall's book report Muhamed as solving (by head work alone, of course) problems like the following: $\sqrt[4]{614656}$ or $\sqrt[5]{147{,}008{,}443}$, though sometimes after a lengthy series of blunders. Krall had, however, at the time of his own writing, already introduced Muhamed to algebra and had obtained correct solutions of equations like the following:

$$x - 48 = 46 \qquad \sqrt{49} + x = 16$$
$$x.4 = 128 \qquad \sqrt{x} + 12 = 18$$

The results in language include the reading of written words and letters (*i.e.*, the translating of them into an arbitrary system of taps with the foot), the phonetic spelling of spoken words, the recognition and naming of objects shown in pictures, the answering of spoken questions, and finally a certain amount of spontaneous expression in simple phrases and sentences. After some practice in the formation of sentences the words "was Zucker" were written on the blackboard. Muhamed tapped *süs*. Told that this was no sentence he tapped *z* [for *Zucker*] *ist*. "Was kannst du noch vom Zucker sagen?" *z ist weis*. "Und was noch? Ueberlege mal!" *z schmkt* [for *schmekt*] *gut*. Asked next day for still another quality he tapped: *z ht 4 ek* [*Zucker hat vier Ecken*]. While note was being made of this Muhamed tapped spontaneously *iohn*. Johann asked: Was soll ich? M. replied *m gbn* [*Möhren geben*] and later, on similar occasions, filled the pauses with remarks of the same sort: *pao b gbn* [Pao give bread],* *iohn hfr gben* [Johann give oats],

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* Pao is the horses name for Dr. Schoeller, for a good while Krall's trusted assistant.

ig m hbn [I to have carrots], *ig z haben* [I to have sugar], (pp. 157f).

Dr. Schoeller reports the following conversation with Muhamed after certain experiments in which Zarif had been intractable. I translate the paragraph, as an indication of the Elberfeld procedure in the investigation of the more complicated phenomena reported, not attempting, however, to reproduce the horse's peculiar orthography. The horse's replies are printed in italics. Dr. Schoeller writes:

"30 Jan., 1911. I ask Muhamed: Why was not Zarif good? M. *because is lazy.* I write on the blackboard: Why Zarif lazy? M. *because he said begin will not know.* As I do not understand what 'begin' means, I rub out all beyond *said* and ask again: M. *begin will not know anything of it.* M. is asked a third time and answers: *begin he will not know.* He persists with 'begin' and I assume now that he means perhaps 'at the beginning of the teaching.' I say nothing, but write on the board: why begin will Zarif not know? M. *because he is lazy.* Then I write, without speaking: how shall we make Zarif good? M. *be good.* I give it up now thinking that he does not understand me. Next evening (31 Jan., '11) I ask Muhamed, since Zarif that morning was again intractable: Why was not Zarif good this morning? M. *because Zarif ugghusra.* Though I do not understand the last word at once, I use it nevertheless and write: Why Zarif ugghusra? M. *because he is stubborn.* Then I say: I do not understand what ugghusra means, tell me it again. M. *unghorsam* (disobedient). Now I write on the board: why Zarif stubborn? M. *because he said no he will not.* I make a great effort to learn from him now what may be done in order to make Zarif tractable again. Muhamed perhaps did not understand me for he answered finally, as yesterday: *be good.* The meaning of this repeated answer is not clear to me since "goodness" on our part has never been lacking. The occasion of the failure to comprehend I came later to believe lay in the form of the question: What shall we do *in order to,* and therefore on the next day (1 Feb., '11) I try to teach the horse the connection between cause and effect. I did not light the light at first and said: One can't see well, how is it? M. *dark.* How ought it to be? M. *light.* After lighting up I write: What do we do in order to make it light? M. *light up.* I take now a picture book, which the horses have often seen, and in which the picture of a bright colored cock gives them particular pleasure as they have given us to understand in different ways. I write on the board: What do we do in order to see cock? M. *open book.* Now I write: Zarif not good, what do we do in order Zarif good? M. *blow have.* I will not venture to decide whether he means by this reply instead of the earlier *be good* to show us the proper way of treating Zarif. It made quite the impression, however, that he had this time gotten the meaning of the question."

It is interesting to learn as we do from another source that Dr. Schoeller has for some time ceased to participate in the experiments and that Krall's statement as to his withdrawal is that Muhamed and Zarif have explained that Dr. Schoeller has become unsympathetic toward them and that they will no longer work with him.

Does this sound like a fairy tale? If so let us leave Krall for a moment and hear the testimony of a psychologist of international standing, Professor Edward Claparède, of the University of Geneva, who after seeing the horses with his own eyes reports his observations in the volume of the *Archives de Psychologie* for last year.

Again I cull an instance here and there. In the afternoon of the first day Muhamed was asked to spell the name of Claparède. He tapped off in his system of phonetic spelling *Klapard.* "But something is lacking," said Krall and Muhamed gave the letter *e.* Later the horse was asked to spell the name of another participant in the seance, Mr. Tauski. The name was repeated to him twice and he spelled Tausj. "One letter is lacking—what is it?" asked Krall. *c* tapped the horse. "No it is a *k.* At what place is it?" Muhamed tapped 4, *au* counting as a single letter in the letter table in use. "And what letter comes after *k?*" The horse gives *i.*

The cube root of 5832 was given correctly in a few seconds. The square root of 15376 (= 124) and fourth root of 456976 (= 26) were given correctly in about ten seconds. In these tests Krall and the hostler had gone out of the room after the example had been written on the blackboard. The other participants were ignorant of the proper answers. Claparède notes, however, that at the same time Krall was unable to make Muhamed understand that he (Claparède) had a carrot for him and that Muhamed ought to go to him for it. With Claparède alone in the room and proposing a simple example like 15 + 7 Muhamed answered wrong and indistinctly.

On the afternoon of the second day Claparède saw the horses alone with Krall, except for one of the grooms who was moving about in the stables near by but was not present at the seance itself. Muhamed was not in good form, gave many false and meaningless answers but did a little work with square roots. In one instance he persisted in giving the answer 746 which was wrong, but gave immediately the correct one 747 after a cut with the whip. Zarif responded better especially in spelling, giving a recognizable approximation to Claparède's name and bettering it on cross questioning. Claparède asked Krall to give Zarif a bit of sugar and ask him to name it. Krall, however, thought to get at the name indirectly and said to him, Was wünschst du? [What would you like?] to which Zarif responded *Müdseinjg* [*müde sein ich, I am tired*]. Following up the questioning Krall asked, Wie heisst du? [What is your name?]. The horse replied *Garif,* using *g* which is No. 33 in the letter table for *z* which is No. 53. "What other letter," asked Krall still behind the door, "could you set in place of *f? V* replied the horse. "Qui est Zarif?" asked Krall in French. *iig* [ich] replied the horse.

Professor Claparède risks, though with some reserve, an explicit statement that in his judgment correct responses were given by the horses under conditions that exclude absolutely the possibility of guidance by voluntary or involuntary signals, and that it seemed certain to him that the horses counted the taps and spelled spontaneously. For others, however, the growing pitch of the marvels has at last overstrained belief—to mention but one great name, Edinger the neurologist—and Claparède himself points out the need of experiments which lead to certainty and not merely to conviction. At a second visit during the present year Claparède attempted to make tests "without knowledge" upon several of the horses, but obtained only negative results, the experiments both "with knowledge" and "without knowledge" showing a similar and very large percentage of errors. *Archives de psychologie,* XIII, 1913, 244-284.

Since this is the case let us return to simpler and more fundamental matters. Let us see what evidence Krall has to offer in rebuttal of Pfungst's generally accepted demonstration of the dependence of Hans on unintended visual signals? Pfungst's argument is made in two stages. He sets out to prove first by means of tests with questions whose answers are unknown to the questioner (procedure "without knowledge") that Hans is not able to think on his own account, but depends in some way on his questioner; and with this established he then goes on to show by means of a variety of tests and experiments that Hans received guidance through seeing unconscious movements of his questioner.

Now it is clear that to refute Pfungst and to establish the case for independent thought on the part of the horse Krall must meet not only the second, but above all the first point of Pfungst's argument. For if he merely shows that there are cases in which visual cues are impossible, he overthrows, indeed, a portion of Pfungst's work, but fails to touch the fundamental point of the horse's dependence on his questioner. To prove the powers of the horse to be independent like those of man this dependence must be definitely disproved and on this point one would suppose that Krall would have trained his heaviest batteries. This he does not do. He busies himself rather with discrediting the hypothesis of guidance by visual signs, and to the piling up of new cases to be explained, but gives comparatively little space to tests "without knowledge," and treats even his own results with such tests in a very summary fashion, devoting but 13 of the more than 500 pages of his book to the special consideration of experiments of this sort and citing no detailed protocols.

In these 13 pages he argues that while right answers in experiments "without knowledge" would furnish positive proof of independent thought on the part of the horse, the failure of the horse to reply correctly or even at all in such cases does not prove that the horse cannot think independently, because the failure may be accounted for by the perversity of the horse, concurring thus with von Osten's explanation which was that when the horse discovers that his questioner is ignorant of the answer he loses respect for him, becomes intractable and answers wrong or not at all. The horse's surprising ability to discover whether the questioner knows or does not know the answer is credited to his very delicate responsiveness; "All horsemen know well," we are told, "how surely a fine horse shares his master's mood."

In the way of facts in support of this explanation Krall has these to offer as to the general "crankiness" of Hans. Even von Osten was not always successful with him. Most would-be questioners failed entirely, a few succeeded with the first question or two and thereafter failed. Hans refused also when the conditions were unusual, as when von Osten questioned with his back toward the horse; or he answered wrong to a stranger and right to his master.

In the matter of particular tests executed "without knowledge," the following are cited: First, certain tests carried out by Dr. Grabow in 1904 and reported at length by him to Krall (though whether these were reported from memory or from a full protocol taken at the time is not stated). In these experiments Grabow stood in one corner of the courtyard, von Osten in another. The questions (simple arithmetical problems) were written on cards and shuffled by Grabow in his pocket. One of the cards was then drawn out at random and held up before Hans without being looked at by Grabow. The horse was then asked whether he had understood the problem, and on his signifying that he had, he was sent to his master who asked for the result and for the numbers entering into the problem. Then finally Grabow examined the card and verified the correctness of the response. Such tests were made on several days and are reported to have been with few exceptions successful; which, however, throws some doubt upon the rigidity of the conditions under which they were carried out, for similar experiments even with Krall were very far from generally successful.

Some of Krall's own experiments were made with more elaborate apparatus and in such a way as to dispense with the assistant. Hans usually answered the first two or three questions correctly and then refused. They probably were not made in great number, however, as such experiments met with opposition from von Osten who disliked to risk habituating the horse in refusing. In a very few cases Hans answered correctly and persisted in his answers when his questioner thought him in error, *e.g.,* in a series in which the cards shown were intended to contain only directions for certain movements to be made, Hans unexpectedly tapped the number 3 and held to it. Examination showed that a card carrying the word "*drei*" had been included by accident. In another case when the experiment was made by two persons, each showing Hans a card carrying a number unknown to the other and Hans giving the sum, Krall's assistant set a zero on his card. Krall had written five on his and on hearing Hans tap five declared the answer wrong, forgetting, for the moment, the possibility of zero being used. In these experiments also it was usually in the first few trials that Hans was right, and, in general, experiments "without knowledge" were more successful when not given in continuous series but interspersed among tests of other sorts.

These experiments and others like them seem to Krall to establish the ability of Hans to solve problems of which the answers are unknown to the questioner, but he finds still clearer evidence for independent thought in the peculiar and erratic spelling of his own horses, arguing that, as no one can foresee the particular one of several ways which they will use on any given occasion, each case of their spelling becomes in fact an experiment "without knowledge." These horses could express negation by shaking the head, by moving it from side to side, by tapping off the word *nein,* either in full or in a variety of distortions and abbreviations, or by tapping simply *n* accompanied by the side to side movement of the head or by using the words *nicht* or *nichts.* Zarif for example had the following variety: *nein, ne-in, nien, nen, nieein, nicht, nig, nigd, nigt, nihjs, nigst, nych, nycht, neigt, ngeit* (p. 165). Similar variability is found also in the spelling of other words.

For Krall this possible variety in the means of expression constitutes proof positive that the horses do their own thinking, because the peculiar forms used are unpredictable. But is it after all so conclusive? Assume for a moment that the horses are intentionally controlled by a confederate; such a variety of response would then offer no difficulty. The particular forms of response would be equally unpredictable to all the company except the confederate. In a similar way if the control were unintentional and unconscious, the particular response would again be unpredictable and yet not independently selected by the horse. And when to this may be added a certain amount of pure blundering both on the part of the horse in following the control and of the controller in effecting it, there would seem to be room enough for all the variety of responses exhibited. And it will not do, of course, for Krall to say that such an explanation is inadmissible because there is no control of any sort, for that would be simply begging the question, the very point at issue being whether or not some form of control exists.

The experience with the horses which Krall next describes as a warning against hasty inferences from the failure of experiments "without knowledge" seems to the present writer one of the most important observations in the book and one which creates a strong presumption against the possession by the horses of anything like abstract numerical ideas. When Muhamed and Zarif were able to answer correctly problems in simple arithmetic written on the blackboard even when these involved eight or ten place numbers, they were found to give wholly erroneous answers to problems involving but two figures when these were presented on cards instead of on the customary blackboard, even when the cards were black and the figures made with chalk, and even when these black cards were held against the blackboard itself, though when the same figures were written directly on the blackboard all trouble disappeared. Krall's explanation is that by long habituation the idea of number had become so firmly associated with the blackboard that figures not on the blackboard were not recognized as numbers. The breaking up of this supposed association, "required," he laments, "an incomparably greater expenditure of time and patience than to lead them [the horses] up to the number concept in general and to reckoning with ten-place numbers." But is it conceivable that creatures with such small powers of dissociation (since dissociation is an essential factor in the formation of abstract conceptions) could ever have grasped the abstract relation of the number system? And does not rather the failure of the horses to recognize numbers under these conditions throw doubt on the reality of their supposedly independent calculations in more familiar circumstances?

One more set of experiments "without knowledge" was carried out by Krall and his assistant, but in a manner so uncritical that to adduce it in a serious argument serves chiefly to throw doubt upon their whole course of experimentation. The experiment consisted in speaking to the horse from a distance by telephone (the assistant holding the receiver to the horse's ear) and requiring the horse to tap off what had been said to him, the experimenters assuming apparently that it was not possible for the assistant also to hear the rhythm and cadence of what was said and thus to receive at least a suggestion of the words used!

With this telephone experiment the chapter on experiments "without knowledge" comes to an end, and does so, in the present writer's opinion, distinctly without the presentation of sufficient evidence to establish, against the methodical series of Pfungst, the essential fact of the mental independence of the horses. It is highly probable that in the case of Krall's horses as well as in that of Hans we have to do with some form of control.

In his dealings, however, with Pfungst's second contention, that the horses are controlled by unconscious movements serving as visual signals, Krall is much more successful. We have Claparède's testimony that the horses gave correct replies when no human being was near, when Krall and all other spectators had left them, and their tapping was watched through a small glazed hole in a stable door. In other instances also, if we have all the essential facts, the difficulties of an explanation on the basis of control through visual stimuli are very great.

What other sense may serve in the place of vision we do not know. Krall asserts that experiments of his own have shown that signals through the ear were not operative in the case of Hans, but does not describe the experiments further. In the absence of details, the possibility of such signals can hardly be surrendered. It will also be recalled that Pfungst made no serious study of the possibilities of unconscious guidance in other ways than through vision. It is certainly premature to propose the alternative of independent thought on the part of the horses or an, as yet unknown "*Konnex*," such as thought-transference, or something of the kind, between the horse and the questioner, until the sense possibilities have been definitely exhausted; nor can the believer in such an hypothesis be brought to book by a demand that he show in advance how these other senses would operate, for that is something which only a special experimental study of the horses could be expected to discover. It is enough that such an hypothesis has not been explicitly disproved; and until it has been explicitly disproved, it must take precedence of that of independent thought on the part of the horses as well as that of "thought-transference."[7] The very small number of fully established cases in which the horses seemed to persist in correct replies, against the belief of the questioner that they were mistaken, probably does not exceed the limits of chance successes.

And thus I am compelled to bring my tale of learned horses to a close without the final solution of the mystery, a dramatic blemish which I regret but am powerless to remove.

There remain, however, one or two general matters of which I should like to speak. First with reference to the minds of horses. To assert that Hans and the Elberfeld horses probably do not think in the human fashion in which von Osten and Krall have supposed them to think is by no means to deny that horses have minds or even to affirm that the minds which they possess are of a totally different stuff from those possessed by man. Most comparative psychologists would, I imagine, accept the hypothesis that the mind of man has come up in the process of evolution from an undeveloped original similar to that from which the minds of animals have come. But this is a very different matter from saying that the minds of horses, which are now fitted to a horse body and a horse life and have back of them untold ages of horse development can by a few months' teaching be transformed into tolerable likeness to the mind of man which is associated with a human body, has been moulded to a human environment and has had a human evolutionary history. Every animal is equipped with a set of fundamental instincts suited to its manner of life, and adaptation in instinct is every whit as essential to physical survival as adaptation in bodily structure. These instincts are the original framework of the animal's mind, and by determining its attention determine the formation of its habits and its responses to all the moulding influences of its environment. That there is some plasticity in these instincts and so some chance for progress does not in the least invalidate the original adaptation of which we are speaking. We shall expect to find horse minds keen and active and showing their greatest feats in matters of importance to horse life, and they will be most capable of training in the same direction. The work of von Osten, Pfungst and Krall, even under the most conservative construction, shows a responsiveness in horses far more keen than had generally been suspected and is a first-class contribution to comparative psychology.

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[7] I do not consider the possibility of intentional guidance, though it is a possibility, for we have almost no data upon which to go. There seems to be general agreement as to the absolute good faith of Krall, but that he may have been played upon by unscrupulous grooms or others is a possibility which cannot be thrust aside on a mere impression that it is not so, but must in the end be definitely brought to the test and proved or disproved like any other hypothesis.

And yet the chief interest of these experiments in the present writer's judgment, lies in the closeness with which they correspond in their general features to the human experiences dealt with by the Society for Psychical Research. We have in both a group of very unusual phenomena obtainable by some observers and imperfectly or not at all by others, phenomena which we are asked to regard in one case as the sign of independent human thinking on the part of the horses and in the other as the evidence of supernatural powers in the medium, phenomena which in both cases very closely resemble results obtained by trainers and magicians through means avowedly deceptive. We have as the chief human actors men of good faith, but strong preconceptions like von Osten or of active and enthusiastic temperament like Krall, to whom the systematic and formal procedure of science has little attraction. We have the same unwillingness—not altogether unnatural—to permit exhaustive experiments by controversial opponents, because they are "unsympathetic to the horses" or because they are apt to destroy for a time or permanently the essential conditions of the phenomena. We have the same attempts to prove the case by testimony instead of by rigid experimentation, especially by the testimony of men of unimpeachable integrity and of distinction in other lines of study, but of no experience whatever in the matters really at issue. The impression is not to be avoided that we are dealing here with real analogies and not superficial resemblances, and that perhaps the delicate responsiveness of horses and dogs offers us a means of bringing some of the mediumistic phenomena into the laboratory in manageable fashion. It may, at any rate, be safely conjectured that when the case of the horses has been followed through to the bitter end we shall know a good deal more than we do now about the finer and supposedly occult workings of the human psycho-physical machine.

THE REMEMBRANCE OF PROBLEMS AND OF THEIR SOLUTIONS:

A STUDY IN LOGICAL MEMORY[1]

By E. O. Finkenbinder

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I. Introduction

The investigation which is here reported has aimed to determine what mental procedures and what mental processes are employed in solving problems of various sorts, and in subsequently recalling the problems and the solutions. Problems of various kinds and of various degrees of difficulty and complexity,—mathematical problems, mechanical puzzles, and the like,—were presented for solution; and after the lapse of a month or more the observer attempted to recall both the problem and his solution of it. At the time of the initial act of solving and also of the subsequent act of recall, he furnished an introspective description of the mental content and the mental procedure to which he had had recourse. It was hoped by this means to throw light upon the processes of understanding and solving problems; but the chief purpose of the investigation consisted in obtaining an insight into the mental mechanism which is employed in the remembering of data which are logically related.

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[1] From the Psychological Laboratory of Clark University.

II. Historical Statement

Numerous investigators have attacked the problem of 'mechanical memory,' but the act of remembering logically related data has received but slight attention. The pioneer experiments of Ebbinghaus[2] were concerned almost exclusively with the learning and the forgetting of nonsense-syllables. It is true that he also employed significant material,—stanzas of poetry,—but he was interested only in the quantitative aspects of his problem, and he made no attempt to furnish a qualitative analysis of the phenomena of memory. And the work of Radossawljewitsch[3] suffers from the same limitation. Although Henderson[4] was interested chiefly in the quantitative relations of memorial phenomena his paper contains a section which deals with the qualitative analysis of the process of remembering. He finds that three factors contribute to the act of recall: a preliminary adjustment of attention, which is comparable to that which was present during the initial act of learning, and which is accompanied by images of that initial experience; a sense of the general meaning of the material which was learned; and an unfolding of details which is a product either of one's waiting for certain cues to lead to results, or of one's inferring the details from the cues and subsequently employing the criterion of familiarity as a test of the validity of the inference.

In a more recent investigation, Michotte and Ransy[5] presented pairs of related words to their observers and after an interval the first word of a pair was re-presented with the instruction that its mate was to be recalled. It was found that this act of reproduction was usually accomplished by means of an intermediate term: a visual image of the object denoted by the word which was sought; a visual image of the position of the word upon the printed sheet; a motor image; an affective process; or a consciousness of the relation which obtains between the stimulus-word and its mate. This relationship may be symbolized by an image, or it may make its appearance in non-imaginal form. The authors believe that imagery plays only a secondary rôle; the image fails to represent the relation in an adequate fashion. The authors point out that the relation is the essential factor in the act of recall; but the introspections of certain of their observers seem to show that the presence of the imaginal symbol ensures the recall of the relation.

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[2] H. Ebbinghaus. *Ueber das Gedächtnis: Untersuchungen zur experimentellen Psychologie.* Leipzig, Duncker und Humblot, 1885. Pp. 188.

[3] Paul R. Radossawljewitsch, *Das Behalten und Vergessen bei Kindern und Erwachsenen.* Leipzig, Nemnich, 1901. Pp. 197.

[4] E. N. Henderson. A Study of Memory for Connected Trains of Thought. *Psy. Rev. Monog. Suppl.* No. 23. IV. 1903. Pp. 94.

[5] A. Michotte and C. Ransy. Contribution à l'étude de la mémoire logique. *Travail du laboratoire de psychologie expérimentale de l'université de Louvain.* Louvain, 1912. pp. 95.

Bühler's[6] investigation of the remembering of 'thoughts' yielded a somewhat similar result. His method consisted in first presenting groups of sentences or phrases to his observers, and subsequently in presenting analogous (but non-identical) sentences or phrases. The observer reported what sentences or phrases of the original group were evoked by these cues and described the mental processes which were involved in the act of reproduction.[7] Bühler's findings led him to posit the existence of a non-imaginal consciousness of relation; the recall of the complete meaning of a sentence may be mediated by a pure 'thought' without the coöperation of imagery of any sort.

Hirszowicz[8] investigated the processes which attend the act of free association upon the presentation of adjectives and nouns, which appealed to all sense departments. She found that when the stimulus-word is an adjective, merely the quality may be first imagined, then the object to which it belongs; as, for example, first a yellow color with spread-out surface was seen, not connected with an object but very bright, then the surface seemed to be rounded and a lemon appeared. For one observer the images came very rapidly and developed into a definite visual image. Kinaesthetic experiences were sometimes localized in parts of the body; but these and organic experiences were very rare. Definite feeling tone was noted in about forty per cent. of all the reactions. The feeling tone arose from the visual image of the stimulus or reaction word;

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[6] Karl Bühler. Tatsachen und Probleme zu einer Psychologie der Denkvorgänge. III. Ueber Gedankenerinnerungen. *Archiv f. d. ges. Psychol.* XII. 1908. 24-92.

[7] It should be noted in this connection that Bühler's published introspections are selected almost exclusively from two of his six observers.

[8] Sarah S. Hirszowicz. Experimentelle Beiträge zur Analyse des Reproductionsvorgangs. (*Inaug. diss.*) Zürich: Selnau, Leemann & Cie., 1909. pp. 71.

as, for example, the stimulus word was salad and the visual image of a salad with green leaves appeared, accompanied by a feeling of pleasure. Often the stimulus word aroused at first a memory complex; as, for instance, a memory of a definite poem, and with it a feeling of pleasure. The emotional states in the course of reproduction play a much greater rôle in women than in men.

Schwiete's[9] investigation of the conscious structure of concepts is related to our problem in that he employed the method of reproduction to establish both the free and directed associations. Words belonging to the various 'parts of speech' were given as stimuli. In a majority of the reactions visual images of objects associated with the stimulus word were aroused. In certain cases whole constellations of ideas appeared which embodied several applications of the concept, or several appropriate conditions. The meanings, or more complete relations were not seen at once but were developed with the constellations. The structure of concepts evoked in recall are, for the most part, images, and 'feelings of familiarity.' These images are generally dim and indefinite, but at times complete visual images do appear.

III. Our Experimental Arrangement

A. *Observers*

The eighteen observers named below took part in this investigation. Dr. J. W. Baird, (*B*); Dr. A. K. Beik, (*Bk*); Dr. L. A. Blue, (*Bl*); Mr. Walter Butler, A. M., (*Bt*); Mr. P. C. Chang, (*C*); Dr. Helen D. Cook, (*Ck*); Mrs. E. O. Finkenbinder, A. B., (*F*); Dr. S. W. Fernberger, (*Fr*); Dr. S. C. Fisher, (*Fs*); Dr. A. N. Gilbertson, (*G*); Miss B. W. Higgins, (*H*); Dr. J. Allen Hunter, (*Hn*); Mr. Douglas Miner, A. B., (*M*); Dr. Roy F. Richardson, (*R*); Dr. Elizabeth Woods, (*W*); Mr. Raymond M. Wheeler, A. M., (*Wh*); Dr. H. P. Weld, (*Wld*); Dr. S. Yamada, (*Y*).

B. *Method and Arrangement of the Experiments*

From February to June, 1912, problems were presented for solution to ten observers, one observer at a time and in a quiet room; and recalls were made one month or more after solving. Each observer made a second recall of each problem at the close of the year (one month or more after the first recall); and after the vacation of three months, a third recall. The observers who participated were:—*Bk, Bl, Bt, C, Ck, Fs, Hn, M, Wh, Wld.*

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[9] F. Schwiete. Ueber die psychische Repräsentation der Begriffe. *Archiv f. d. ges. Psychol.*, XIX., 1910, 475-544.

A second series of problems were solved during the year 1912-13 by the following ten observers: *B, F, Fr, Fs, G, H, R, W, Wh, Y.* The problems were not presented in the same order to more than two observers,—the changed order being introduced for the purpose of equalizing the possible influence of one problem upon another. For example, if under this irregular order, all observers in attempting to recall a certain problem, recall first the problem that preceded it in the series, we may be sure that this course of association in recall is a general phenomenon. If, however, this course held true for two problems that had been presented in the same order for all observers, it might still be due to inherent qualities of the problems and not to the relative position of the two in our order of presentation.

The materials were presented in either a visual or an auditory fashion. Those statements of problems that were read by the observers were either typewritten, or written in the experimenter's hand-writing. Our interest here was to determine whether the course of recall is the same for the three modes of presentation. (See p. 72.)

The observers were instructed, in certain cases, to read the statement carefully, then to introspect upon the perception of the problem up to that point. In other cases, they were allowed to proceed with the solution without introspecting upon the initial act of apprehending the problem. When the observer had made a step or a number of steps,—which fact was apparent to the experimenter from the observer's actions, statements, or penciled notes,—the experimenter interrupted with the request, "Introspect," or "Introspect upon your last step." It frequently happened that the observer had become so deeply interested in the solving that he turned with reluctance to introspect. On the other hand, at times his attention turned immediately to the introspection, and he began his description voluntarily. The reader may object here that in thus introducing interruptions, we have interfered with the observer's natural procedure in the solving of the problems. But if the solving be carried through from beginning to end without a break, the mental processes are often so numerous that a complete introspection of the whole series at once is impossible. We are interested in determining just what processes occur in the solving, in order that we may determine in the subsequent recall what proportion of the processes are recurrences of the steps in the initial solving. Without a detailed knowledge of the processes occurring in the solving we can not determine what is recalled, and this method of introspection seems most appropriate.

The uninterrupted introspective report was written as dictated by the observer; then questions concerning unclear description of structure and of function were asked by the experimenter. These questions were also recorded in every instance. In addition to the recording of introspective reports and questions, we noted the remarks, gestures, or other expression of the observer, as well as the successive steps pursued in the solution. In the case of one observer, *Wh*, plethysmographic and pneumographic records were obtained; these, however, contributed little or nothing to our investigation, and they will not be described or discussed in our statement of results.

It turned out that certain observers failed to solve some of the problems assigned. In such cases, the experimenter furnished the solution after an hour had been spent in an unsuccessful attempt.

The observers were instructed not to attempt a recall of the problems at any time between sittings; they were also instructed to make an effort to direct their attention elsewhere in case the problem began to intrude itself into consciousness during the intervals between sittings.

In the reproduction of the initial group of ten problems, the observer was first asked to recall the first problem that he had solved; and at each subsequent sitting, the problem next in the order of solving. This method affords information concerning the particular factors by which the problems were ushered again into consciousness, in that the experimenter presented no datum of any problem to the observer as a clue to recall. If in his act of recall, the observer hit upon a problem, which had been presented later than the one called for, he was not informed of his error, but was allowed to proceed with a complete recall of that problem, and of its solution. If he were unable to recall the problem or its solution in detail, he was given certain cues, or was asked questions which directed his attention to various points that had been overlooked. These cues and questions were recorded, as were all other questions asked by the experimenter.

In the later experiments, cues were given for the beginning of a recall, as, for example, "Recall the density problem." (During the solving, the name "density" had not been given to any problem, although several involved that quality of matter.) Cues both of an abstract and a concrete nature were given, in order to enable us to determine their relative value in the act of recalling the problems to consciousness. (See p. 70.)

The method of recording the results in recall was the same as in the initial solving, as already described, except that when no farther recall seemed to be forthcoming, additional cues were given. The observer was encouraged to guess and to fill up the gaps in his recall; and this frequently led to a statement of the problem or its solution. When he wholly failed to recall the problem, it was presented for re-solution. This method furnishes data for a comparison of the courses taken in the two solutions.

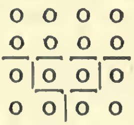
It will be found that our problems are more difficult than those which have been employed by any other investigator of this topic. It will be found, too, that the intervals which elapse between the act of learning and the act of recalling, in our investigations, are relatively long. And our method differs from that of former investigators in that we have made an attempt to discover the structure as well as the significance or function of the mental processes which occur in the act of solving problems. Every act of memory is concerned, of course, with a mental content which has been acquired at some previous time; and in any investigation of memory, it is essential to determine just what was present to consciousness during this initial act of acquisition; without this datum it is obviously impossible to institute any comparison between original experience and subsequent recall.

C. *Materials*

Our materials consisted of thirty-two 'problems' which had been selected as appropriate for our investigation. They gave rise to a wide variety of mental procedure and mental content, on the one hand; and on the other hand, the steps in mental procedure, during solving and during subsequent recall, were not of too long a duration to admit of detailed introspective analysis.

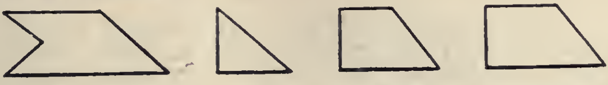
These problems as quoted below were presented to our observers; the name which appears here within parentheses is appended for later reference in our exposition.

1. (*The Dot Problem*)



"Readjust the lines so as to make groups of 6, 6, and 4, instead of the present four groups of 8, 3, 3, and 2. You must move only two of the lines in order to attain this end."

2. (*The* T *Problem*)



"Assemble these pieces of cardboard to form a capital T."

3. (*The Ring Problem*)

This mechanical puzzle consisted of a small hexagonal plate of vulcanite, and a ring of heavy wire. The plate was pierced by seven holes, and two additional holes had been bored half-way through it. The two ends of the wire which constituted the ring were not exactly opposed, but were slightly displaced in a lateral direction. The plate, with the ring passing through one of the holes was put into the hands of the observer with the instructions that he should remove the ring from the plate. The problem could be solved very simply by rotating the ring, threading it through several holes in succession until it was finally passed through the central hole, where by tilting the ring, it could be detached from the plate.

4. (*The Touch Problem*)

A sheet of paper was laid upon the observer's forehead; two or more dots were then made upon the paper. The observer was then asked to transcribe these dots, in correct order and position, upon a sheet of paper which lay before him on the table.

5. (*The Memorizing Problem*)

"You will be given one half-minute to memorize the numbers on this sheet."

8 22
15 29 1
26 47 62
4
33 100

The page of script figures was presented to the observer and after he had memorized for one half-minute he was called upon for his general method of procedure. Then a second half-minute was spent in memorizing, after which a complete introspective account was given.

6. (*The Memorizing Problem—Division*)

A sheet containing the following group of numbers was presented with the same instructions as in Problem 5. The second half-minute was spent in noting the numerical relations and in memorizing.

5
27 16 7
21
12 13 48
18
19 10

(Problems 5 and 6 were designed to bring to light the differences between the recall of simple, isolated data and the recall of numerically related data.)

7. (*The Addition Problem*)

"Choose three of these numbers whose sum is 50."

3
25 27
15 6 14
43 9 31 19

8. (*The Counters Problem*)

Five cards, each 5 cm. by 7.5 cm., labelled A, B, C, D, E, and twenty counters 2.5 cm. in diameter, numbered consecutively from one to twenty, were presented with the following instructions: "Place the five cards on the table, A to your left and away from you, B about 5 cm. to its right, C midway between the two but nearer to you by the length of a card, D to your left with its furthermost corner at the right near the closer left corner of C, and put E to the right of C as D is to the left. Place the ten counters in numerical order on C, one at the top and ten at the bottom. You are required to move the complete pile of counters to another card, one at a time, never placing any counter on one bearing a smaller number than itself. *a.* What is the fewest number of moves necessary?"

b. "In how few moves may a pile of counters numbered three to thirteen inclusive be moved to a second card in like manner?"

c. "In how few moves can a pile of twenty counters be placed on a second card in the same manner?"

d. "In how few moves can a pile of five counters be placed on a second card in the same manner?"

e. "In how few moves can a pile of fifty counters be placed on a second card in the same manner?"

9. (*The Box Problem*)

"What are the dimensions of a rectangular box whose top contains 120 square cm., whose side contains 96 square cm., and whose end contains 80 square cm.?"

10. (*The Weight Problem*)

"A man had a forty-pound weight which he broke into pieces of such sizes that they enabled him to weigh any number of pounds from 1 to 40. What did the pieces severally weigh?"

11. (*The Hog Problem*)

"A farmer, when asked the weight of a certain hog, replied, 'Just seven score pounds and a half its weight.' How much did the hog weigh?"

12. (*The Spider Problem*)

"A spider is on the end-wall of a room, one meter from the ceiling, and five meters from each side. The room is 10 meters high and 20 meters long. A fly is one meter from the floor and five meters from each side, on the end of the room opposite the spider. The spider crawls on the surface of the room to the spot where the fly is. What is the shortest route? It is less than 30 meters."

13. (*The Triangle Problem*)

"Outline[10] all the kinds of triangles you know."

14. (*The Number Arrangement Problem*)

$99\ 9/9 = 100$; $(5+5) \times (5+5) = 100$. Find other similar combinations of digits that equal 100."

15. (*The Hat Problem*)

"In how many different ways may two men exchange hats? Three men? Four men? Five men? Six men? Seven men? Eight men? etc. Find a rule for determining the number."

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[10] The word "outline" was here employed ambiguously to permit the observer to solve the problem according to either or both meanings. Our interest was to determine how the meanings are recalled.

16. (*The Multiplication Problem*)

"Suppose in a multiplication problem the nine digits are used in multiplicand, multiplier, and quotient, *i. e.*, four in the multiplicand, one in the multiplier, and the remaining four of the nine in the quotient. Choose the numbers for multiplicand and for multiplier so that the quotient will be the four digits that you have not used in the multiplicand and multiplier."

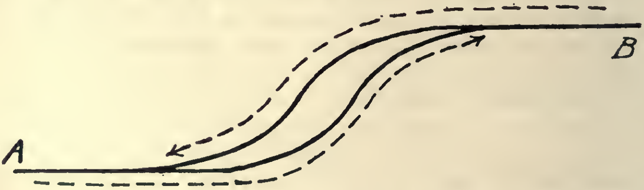
17. (*The Apple Problem*)

"I paid a shilling for some apples, but they were so small that the dealer gave me two additional apples. I find that this made them cost just a penny a dozen less than the first price asked. How many apples did I get for my shilling?"

18. (*The Problem of the Dutchmen*)

"Three Dutchmen, named Hendrick, Elas, and Cornelius, and their wives, Gurtrün, Katrün, and Anna, purchased hogs. Each bought as many as he (or she) paid shillings for one. Each husband paid altogether three guineas more than his wife. Hendrick bought twenty-three more hogs than Katrün, and Elas eleven more than Gurtrün. What was the name of each man's wife?"

19. (*The Problem of the Two Trains*)



"Two trains, each containing sixteen cars and an engine, met as the diagram indicates. Each switch will hold just seven cars and an engine, or eight cars. The trains pass each other and move as shown in the diagram. How do they do it? How many reversals of each engine?"

20. (*The Angle Problem*)

a. "Determine how many degrees in an interior angle of an equilateral triangle;

b. of a square;

c. of a regular hexagon;
d. of an octagon[10a]
e. of a pentagon;
f. of a decagon;
g. of a regular sixteen-sided figure."

21. (*The Milkman Problem*)

"A dairyman in preparing his milk for public consumption employed a can marked B, containing milk, and a can marked A, containing water. From can A he poured enough to double the contents of can B. Then he poured from can B into can A enough to double the contents of the latter. Then he finally poured from can A into can B until their contents were exactly equal. After these operations he sent the can A to London. The problem is to discover what are the relative proportions of milk and water, that he provides for the Londoners' breakfast tables."

22. (*The Density Problem,—Fresh-Salt Water*)

"Does the water-line rise or fall as a boat passes from fresh to salt water?"

23. (*The Train Problem*)

"I asked a farmer how far he had to drive from the station, and he replied: 'If I get out at Appleford, I will have as far to drive as if I had gone to Bridgefield, fifteen miles farther on; and if I change at Appleford, and go thirteen miles to Carterton, I will still have the same distance to drive. Carterton is fourteen miles from Bridgefield.' How far did the farmer have to drive?"

24. (*The Density Problem,—Density of the Earth*)

"If the earth were of equal density throughout, it would be about two and a half times as dense as water; but it is about five and a half times as dense; therefore, the earth must be of unequal density. What is your reaction?"

[10a] The word 'regular' was omitted in the statement of the sections d, e, f, of this problem in order that the observer would be presented with an impossible task if he comprehended the statement of the problem. In this way an additional factor was presented. Several other problems contained similar situations. Any confusion which might arise therefrom was desirable because it gave opportunity for determining the readiness of the recall of this factor. (See p. 76.)

25. (*The Water Problem*)

"How much deeper is a body of water than it looks?"

26. (*The Vision Problem*)

"The *minimum visibile* is the least magnitude which can be seen; no part of it alone is visible, and yet all of its parts must affect the retina in order that it may be visible; therefore, every part of it must affect the retina without being visible." Is this true?

27. (*The Nephew Problem*)

"How can each of two men be uncle and nephew to the other?"

28. (*The War Problem*)

"The object of war is durable peace; therefore soldiers are the best peace-makers. Do you agree?"

29. (*The Cramming Problem*)

"Cramming is detrimental rather than otherwise; for I have noticed that, no matter what the subject is, I invariably write a poor paper when I 'cram' and a good one when I do not cram. What is your inference from his experience,—cramming is detrimental or not detrimental?"

30. (*The Proposition Problem*)

"Outline the kinds of propositions as used in logic."

31. (*The Aristotle Problem*)

"'Aristotle is living' and 'Aristotle is dead' are both intelligible propositions; they are both true or both false, because all intelligible propositions must be either true or false. Explain."

32. (*The Linnets Problem*)

"What fallacies are contained in the following? Linnets when shut up and educated with singing larks,—the skylark, woodlark, or titlark,—will adhere entirely to the songs of these larks instead of to the natural song of the linnets. We may infer, therefore, that birds learn to sing by imitation, and that their songs are no more innate than language is in man."

IV. Results

The chief purpose of our investigation has been to analyse and describe the mental processes involved in the act of recalling logically related materials,—problems, and the observers' solutions of them. We shall treat individually, in section *B,* the recall of a number of the problems, in the order given above. From their descriptions it is hoped that the reader will be able to see indications of general tendencies. These tendencies as indicated by the total number of single recalls (600) will then be treated under separate headings in section *C.* Since a description of the act of recall would be unintelligible to a reader who did not possess a knowledge of the observer's act of solving the problems, it will be necessary to preface our statements of the results of recall by a brief statement of the processes employed in the act of solving. In section *A,* then, we shall describe a few of the general features of the solving. But in order to make clear the statement of results of recall in sections *B* and *C* a number of the particular facts of solving will necessarily be treated there.

A. Solving of the Problems

For the solving of the earlier problems in our list, we presented the physical objects so that the observer might perceive and manipulate the parts to aid him in his thinking. The solution of these problems depended upon the perception of the relations existing between the parts of the object presented. In fact, all of our problems are similar in this respect; but those later in our list involve many relations which deal with human experience. They are considered to be more abstract and more difficult by most observers.

In not a single instance did any observer solve a problem without having recourse to some physical object, which he either perceived or imaged. It is interesting to note that most observers examine this object of perception with considerable care, even checking up incidental features before attempting to solve the problem. In the presentation of the 'dot problem' (see p. 38, problem 1), the number of circles employed are given both in the description and in the diagram. Nine of our eighteen observers first of all added the numbers in the groups and counted the circles in the diagram as a check upon the statement of the problem. Only after this superfluous preliminary step did they begin their attempts to solve it.

All but two of our eighteen observers made at least half a dozen definite trials which they wrote, and often many more trials in visual imagery that they did not write. At first they attempted to find a variation from the original which would constitute the solution; ten then attempted to write the possible combinations of the sixteen circles in groups of six, six, and four. No one continued this line of attack to its logical end, but returned to his first method, because to make all the possible combinations of six, six, and four became as tedious in itself as the work of moving two lines in every possible way. When they tried to match up these groupings of six, six, and four, with that presented, they found that they had employed different numbers of lines, and had never used the same number as there were in the original grouping as presented. Further, in some of these forms, not a single line was in the same position as in the original grouping. The observers perceived this extreme variance, and they also saw that to form such a grouping every one of the nine lines in the original must be moved; consequently they returned to the attempt to move two lines to the proper position. By a long process they eliminated many possible moves until they finally hit upon the correct ones. The two other observers perceived more relations and began their attempts at solving in a more rational manner.

It is not an easy matter to distinguish sharply between the use of trial images or assumptions, and the act of reasoning One might extend the meaning of the term 'reasoning' to include all trial solutions; but as we shall use it in this paper, it will embrace only those trials in which the observer foresees the necessity of the step, or in other words, when he sees the result of a step before he takes it, opposed to those trials in which he works without seeing the necessity for a single move. Only two of our observers, during the solving, used reasoning in this sense of the word. The steps of reasoning that these observers attended to were, first, the fact that the two circles in the center group must be allotted to another group in order to make groups of six, six, and four; second, the question as to which line of those bounding this group of two should be moved. Then they saw that if the lower boundary lines be left unmoved and the central dividing line below be removed, one group of six is determined; and they then concluded that one of the two lines on the upper boundary may best be removed. This leads directly to the solution. The following introspections are examples of reasoning in the first solving.

Ex. 1.[11] Obs. *Wh.* [The dot problem. 65 sec.] ("Introspect.") "My attention fastened upon the group of two, and I saw that in order to make groups of six, six, and four, one of the boundary lines of this group would have to be removed. I looked to see to what group these two should be added. Then I saw that by removing the lower line the two groups of three each would be connected and form a group of six, so the two circles should be thrown open above and be added to the group of eight. And this I imagined as done by viewing it as if just one of the lines were removed. . . ."

Ex. 2. Obs. *Ck.* [120 sec.] "I went at it pretty systematically to see how many of the heavy lines could be left unmoved. I saw that the top group had to be divided somehow. Then there came visually,—six below by removing only one line [the lower vertical line]. A top boundary line then had to be taken from the center group of two. I imaged each line taken. Now I had the center group broken and also the lower group of six determined; and I took just a moment to see where the two lines should be inserted in the large group of ten above. I saw the four at the left and placed the lines vertically in the center, finishing the problem. . . ."

A majority of observers did not at first perceive a relation that would bring them to a ready solution; instead of so doing they fastened their attention and directed their manipulations upon some unessential relation or false assumption. For instance, in the case of the 'block T,' the right angle was the first thing that the observers tried to fit or fill in. This kept them busy a long time and when they found that they could not accomplish it they concluded that the T could not be made. A new assumption could not be found and so a sheer trial and error method of fitting the pieces was begun with the hope that they would soon fall together in such a way as to look like a T. Only one observer perceived the relation and proceeded to work on the basis of this observation, that an edge of one of the pieces must necessarily fit upon or match an edge of another piece. This method brought the solution in a minute, whereas the other observers worked for ten minutes or, in some cases, almost an hour. This same difficulty of finding a new assumption or method of solving was present throughout all the problems. The essential feature in the solving of any problem is the attending to a particular portion or aspect of the physical object about which the problem is built. But ordinarily the observer fails to do this and some relation is overlooked, which must be seen if he wishes to reach the solution methodically and not accidentally.

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[11] The numbers appended to the examples of introspection are inserted for future reference. The quotations within parentheses are the experimenter's words; the quotations not in parentheses are the introspective reports of the observer. Insertions by the experimenter for explanation are enclosed within brackets. The time in each case before the beginning of the observer's response is noted in seconds.

B. The Recall of Individual Problems

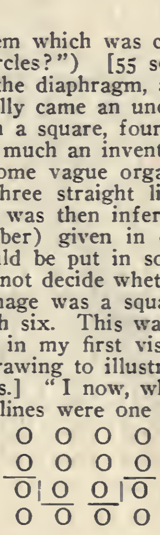
Problem 1. (The 'Dot Problem.') It will be our purpose here to present particular facts of the recall of problem 1. These are: (1) the cues necessary to evoke a recall; (2) the time spent in arousing the first datum of recall; (3) the structure of this datum; (4) the nature of the first imagery; (5) aids employed in definitizing this imagery; (6) stability of the later definite images and the significance of this stability; (7) the dominant kind of imagery; (8) trial images and their aid to progress; and minor points of detail. This treatment, it is hoped, will present to the reader a definite view of the act of recalling.

Although this problem had not been named when presented, and actually is not a dot problem, the observers were given the cue "Recall the dot problem." The name 'dot problem' was used here in order that the observer might be started on his recall without being given a single factor of the problem. In only four cases was this cue insufficient, whereupon the words "The circles problem" were added. Three observers recalled the problem in all its essential details, except for the fact that they reproduced dots instead of circles. One recalled this problem when asked to recall the 'box problem;' for her the groupings were considered as boxes. In one case, in addition to the cues 'dot problem' and 'circle problem' it was necessary to add another cue, which was 'the groupings.'

In no case was there any immediate memory that such a problem had been solved. The length of time between the request to recall and the first traces of recall was never shorter than several seconds; in certain instances it was about five minutes. In these slower reactions, the observer reported an initial feeling of familiarity and a stage of warming-up; then a vague, schematic, visual image. This image slowly became more definite and took on detailed characteristics, such as lines, circles, groups, etc. This progressive development of the process of recall is illustrated in the following introspections:

Ex. 3. Obs. *Fs.* ("Recall the dot problem.") [5 sec.] "There was almost immediately a rather slight tension. The focal things in consciousness were the sound of your voice which lingered in consciousness, and the vocal-motor innervation to say 'dot,' with questioning inflection. I actually whispered that word two or three times. Then a single puzzle occurred to me, but I cannot now remember what it was. It was dismissed almost immediately as not having anything to do with dots. Visual image of that diagram of the train puzzle, that is, the paper with broken lines appeared; but that also disappeared almost instantaneously. The star puzzle appeared, connected with the word star, as a visual image. The puzzle was ten stars in lines, but I simply saw the ultimate solution. That also disappeared immediately. [This latter is not a problem of our investigation.] I did not recall the problem." ("Can you get it now?") [22 sec.] "Just a moment ago I had a flash of familiarity and nothing more than the vaguest sort of a visual image of dots, indefinitely located in space,—a faint recollection that I had had such a puzzle before. It was not present so clearly as that,—only a few dots arranged in a square, perhaps sixteen in all. The visual image was almost instantaneous, but of increasing clearness. . . . . I then started out with familiarity and these dots, together with a verbal motor, half-formed sentence, of which the general purport was: 'Those dots are to be divided by a certain number of lines into certain definite groups'; but I was too intent on going ahead with the problem to have that sentence come as completely as I have given it. Lines attracted my attention so that I did not seem to go on to work out some way in which this thing could be divided off. I immediately set down these dots in that square and found the tendency to divide them off in various ways with lines. Nothing systematic about that because I was unable to recall the way they actually had been divided off. A tendency to try and divide them into equal parts. I abandoned the effort and began to introspect. . . ."

Ex. 4. Obs. *Ck.* ("Did you ever have a problem which was concerned with dots?") [30 sec.] "No . . . (Or circles?") [55 sec.] As nearly as I can say, there was a relaxation of the diaphragm, and perhaps a change in breathing. Then there gradually came an uncertain visual image of small black circles arranged in a square, four or five each way. At first this image seemed to be as much an invention of my own as anything else. Then there came some vague organic reaction; then came a visual image of two or three straight lines dividing off these dots from others. The problem was then inferred to be: three lines (I am not certain of the number) given in one position; I am to move them so that the dots could be put in some sort of grouping, I do not know what, at all. I cannot decide whether there were four or five dots in a side, but each image was a square. I think the final grouping had something to do with six. This was a pure inference from the arrangement of the dots in my first visual image." [She called them dots, but now in her drawing to illustrate her visual image, she drew them correctly as circles.] "I now, while drawing, recalled vaguely (visual image) that the lines were one for each dot. This was at first very indefinite. The dots going around a square corner were counted in one group. This is the lower six. The dots in this arrangement now came out clearly, simultaneously.



Ex. 5. Obs. *M.* ("Recall the problem that you solved after the counter one.") [40 sec.] "I had a series of visual images, in the following order; the discs, the railroad, the capital T, and the circles.

These were located in a visual schema, rather than detailed images. Then at last the circles in my visual image cleared up. At first they were dim—perhaps three rows of four each. When I started to set them down they cleared up nicely into four rows of four each. The heavy lines began coming in. . . ."

Ex. 6. Obs. *Bk.* ("Recall the dot problem.") [35 sec.] "I imaged the typewritten sheet with the diagram on it. Can almost read the statement of the problem. A '6' and an '8' come in. My visual images are so dim that I do not know whether they are constructed or recalled. I am not sure that there were four dots in each line and each column." [Draws.] "Oh, the heavy lines come in, and they make the whole figure lopsided; also a lower line keeps coming in. These images are multitudinous and as involuntary as reversible visual illusions. . . ." [The lopsided figure which came in at this point was in reality a visual image of the correct solution, as was clearly shown when Bk drew a picture of this visual image.]

Ex. 7. Obs. *Wh.* ("Recall the dot problem.") "Immediately I got a vague image of the original paper with dots on it,—with no definite arrangement, however. The first image was more like dots than ciphers. Now I see ciphers. The figure that I first saw was in the shape of a cross, then it became a square. [Pauses 2 seconds moving lips as if counting.] I cannot tell you the number; I will draw it." [While drawing he said:—] "There were 12, I think." The fact that there were sixteen circles did not occur to my consciousness until they had been drawn. Just after I had drawn them I had a visual image of the lines within the figure. . . ."

The number of circles was never definitely recalled at first. The observers often began to record dots and then recalled that dots had never been seen, but circles instead. Also the number of circles was definitized in writing them, as is shown in introspections 5, 6 and 7. Usually the visual image preceded the writing; but it was very often verified or corrected by the perception of the trial forms that the observers wrote on the page.

The above examples of procedure in recall indicate also the nature of the imagery in regard to its instability and its development. We see a growth in clearness and detail, also a correlation between clearness in details of the image and recall of the steps in the solution. This is more or less typical of all eighteen observers. In Ex. 12, p. 53, the haziness of some of the lines signified that these had been the ones shifted in the course of solving. At times the images were stable and slow in development; but more frequently, particularly in the cases of *Wh, Ck, Fs, Bl,* and *Bk* they were multitudinous and very unstable, as in Ex. 6, given above, and in Ex. 8.

Ex. 8. Obs. *Bl.* "I have recalled the general form of the problem in indefinite imagery. Can see fluctuations of groupings but cannot say what they are. Am sure that the problem is made up of circles, divided into groups by heavy lines, but cannot see how many of either, or their exact position, because my imagery is too vague and fleeting. . . ."

In almost every case the images that appeared at a time when the observer was attempting to recall were very significant to him. Aided by these, he recalled or constructed others which formed a problem,—often the correct one. The following introspections are examples of this.

Ex. 9. Obs. *C.* [60 sec.] "Just a vague visual image of the lines. The problem is to move two short lines so as to divide the spaces into 2, 6, 1, 4 instead of . . ., Wait just a moment. I am about to get it now." [He draws 1 min.] "All that I remember clearly is that I moved two lines; I see myself moving them. However, I am not very sure just where these two moves were made." [Pauses 5 sec.] "I have a vague visual image of a written 4, 6, 8; no, it came more in vocal motor imagery. It seemed to come spontaneously with the problem itself. The moved lines came in quite clearly." [Here he drew the position of the two lines when the problem is solved.]

Ex. 10. Obs. *W.* ("Recall the dot problem.") [13 sec.] "At first no more than the word 'dots' shifting in and out, auditory imagery; and then black dots faintly coming and going in visual imagery. Just now there comes a visual image of dots arranged in a sort of cross, and vertical lines appear between two of them. This brings the certainty that the problem is to group dots, the certainty being merely the fact that it is a fixed (stable) image with fixed meaning. . . ."

Certainty in recall is apparently closely related to stability of images, as is illustrated above, in Ex. 10; but it is not safe to generalize from recalls of a single problem, by only eighteen observers. Later (p. 72) we will discuss this point, and also the factor of meaning as these are found occurring throughout the various problems.

The visual was the dominant mode of imagery, in the recall of this problem; this was to have been expected, because little use had been made of other kinds of imagery during the solving. In addition to the visual imagery in the solving however, every observer employed words, and movements of the hand in drawing the trial positions of the lines; but the solving was carried on almost exclusively in visual imagery. In the recall the only imagery besides the visual was the hand motor, or vague kinaesthetic, which was bound up closely with the visual. Only three of eighteen observers noticed this kinaesthetic imagery.

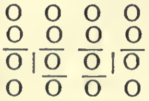
The trial groupings that had been made during the solving (p. 46), never came clearly to consciousness again during the act of recall. In half of the cases they were recalled as vague visual images, so dim that not a single line or group was seen distinctly. In a few cases, the remembrance that many unsuccessful trials had been made in solving was merely a vague memory of the time spent in the room trying to solve the problem upon the first day. The general situation in the room was most definitely remembered, together with the visual image of the solution and the fact that the problem had been solved.

The observer who, in the act of recalling, has an abundance of trial images is the one who makes the most rapid progress. But these are never the same images that occurred during the solving. It is not surprising that this proved to be the case because not only did his present goal lie in a different direction but he also set out from a different starting-point. Only one of the hundreds of trial images that had been employed in the solving was recalled, and this incidentally, after the essentials of the problem,—the form of the final solution, the presented form and the complete manipulation which was appropriate for the act of solving,—had been recalled. Not even the steps in reasoning which appeared during the solving were reinstated, as will be shown below.

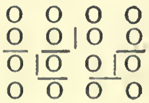
Neither of the two observers who used the steps of reasoning in the solving (p. 46) recalled them, but recalled first the visual image of the solution and then the image of the form originally presented, with the attention directed to the two lines and their change of position from the original to the final form. Hence they recalled what they had seen and done; they did not recall the reasons why these particular moves should be made.

First recall showing the failure to recall the steps of reasoning given in Ex. 1.

Ex. 11. Obs. *Wh.* [Continuation of Ex. 7.]
[His drawing]



"This is the original presentation, and the problem was to make the groups six, six, and four instead of eight, six, and two. I had a visual image of the typewritten figures 6, 6, and 4. I think my instructions were: Change any two lines. This comes to me now in the visual images of seeing myself move them." [Pauses 90 sec.] "I think I have an image of the finished figure."
[He draws]



[Pauses 30 sec.] "What troubles me is to get the lines there by two moves." [Studies 150 sec.] "I have it,—another line below in the original visual form. This was slow to come in, although I attended strenuously to the first drawing above, trying to make it clearer in visual imagery, and to see another line in it, which I might place vertically above in the center of the final drawing."

First recall, showing the failure to recall steps of reasoning given in Ex. 2.

Ex. 12. Obs. *Ck*. [Continuation of Ex. 4.] "In this visual image there were one or two lighter lines fleeting down below somewhere. To interpret, the fact that they were lighter seems to indicate that they do not belong there permanently. The dim gray lines tended to move into the black ones and stay there. This seems to represent the problem and the solution." [Pauses 84 sec.] "My first thought was to move these two lines up here (copying my visual image from above). My act of solving consisted in moving the lower line visually, and that leaves six immediately. Another scrap of memory is that the two lines, after being moved, stand end to end. This came in first as a visual image." [Pause 35 sec.] "Can it be possible to divide the circles into four groups of four each? I have been trying to imagine lines to do it, and to see if they looked natural that way. I did this in three or four ways very quickly. All I am clear about is that one or both of the two center dots is to be counted with some others,—a remnant of the first definite visual image and an imaging of how I can make it conform. I am surest of the general longness of the group. I cannot get any other familiarity clue. Each construction looks as unfamiliar as any other, so I do not know which, if any, is correct. I could reconstruct the problem by making groups of six, six, and four, and taking the smallest number of moves necessary to make this number. From the memory image of the two vertical lines in the center above as the final solution, and the lower group of six, I get the original form; and I assume (visual image) that I got one of the vertical lines from below. That makes it six, six, and four. . . ."

We may call this last example an act of reasoning from a basis of two definite memory images to the formulation and solution of the problem. But these steps are not the ones which she took during the first solution (Ex. 2, p. 47). Several other clear cases of reasoning occurred only in the recall of this problem, and these consisted in filling in similar gaps between the bits of definite recall.

Recognition often resulted when the gaps were properly filled, as the following example shows.

Ex. 13. Obs. *M*. [After drawing the final and the original forms as he had constructed them by filling in the gaps between his memory images, the following introspection was given.] "The two circles were shut off in the center, I remember visually. I think that we were to change two lines so that six circles were inclosed in every pen. The most clear is the group which I draw here. I read out of my diagram that there were groups of eight, six, and two at the beginning. The first six is remembered in the form of the visual image of the group. The clearest part of my remembrance of the original presentation of the problem is the diagram, and it is not yet definite nor certain. The 'two moves' was auditory as if I heard you reading it.[12]

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[12] "I have not now been able to recall the words visually." [It is interesting to note that the statement of this problem had been read to the observer for the first solving.]

> "After I had completed the figure I tried to prove it to see if two moves were necessary. It does not work. I see I have eight lines in the first, and nine in the last drawing. I have not thought which lines of the original figure were moved to make the final form. The figures were separate visual recalls and not related to each other as they would be if the one were formed from the other by the shifting of two lines. Now I have the impulse to insert a line into the center group between the two circles. Oh, no. There it is vertically at the bottom" [dividing the group of six into two groups of three each] "This is very convincing, and is verified by the image I now clearly recall of it. . . ."

The above examples show that the recall of this problem has been a keenly active process, and that there was much searching and striving to recover the problem and its solution. As was stated earlier, the observers employed much imagery in recall that was of a constructive sort, in order to see if any familiarity would attach to these created images that might aid in recall. Many observers found this procedure very profitable, and every one used it to some extent.

Every observer busied himself unintentionally first of all with the recall of the solution; and in every case, the first definite thing to appear was the visual image of the drawing, or a part of it, that they had made in their solution. The visual image alone constituted the structure of this recall. Rather surprisingly, this visual image, in almost every case, was recognized as the solution, from its being seen in one's own handwriting, on the page at the end of a number of vague drawings (unsuccessful attempts at solution), sometimes from seeing one's self leave the room, or just getting the solution as the time was up, from its familiar toning, from one's feeling of satisfaction, or from a relaxation of attention that occurred with the appearance of this image of the solution. When this point had definitely been settled, the procedure tended toward the recall of the original figure. Seldom was this attained by pure recall, but most often by a process of constructing from the recalled images. Every observer proceeded in a direction opposite to that which he had followed in solving, the recall terminating at the point where the solving had taken its start.

Certain individual variations have been mentioned in the discussion of this problem but they are not of sufficient importance to merit further treatment here.

2. The Block T, and the Ring Problems. (See p. 39, for the statement of these two problems, both of which are mechanical puzzles; and see p. 47, for the statement of the method of solving.)

After an interval of a month or so, the recall showed the following main points. An assembled T, approximately the size of the original was clearly recalled, then the lines uniting its parts appeared slowly,—all in visual imagery. These images did not become clear however until after the attention had fastened upon the right-angle at the juncture of the stem and the crosspiece. This angle was recalled as the jog in the end of the large piece, whose location in the T had been difficult to discover, but, when once found, made the assembling a very simple and easy matter. The lines were then seen very clearly in the visual image. The word "bias," which carried the meaning of the crux of the problem for one observer, was recalled first (visual image) by him. Another recalled the diagonal lines, which meant for him that they were the essentials of the problem. Never were the pieces severally recalled, in definite form, previous to the appearance of the image of the assembled T. Five of the eighteen observers, who had recalled the essential features of the problem, were unable to recall or to re-construct the pieces of the T. Nearly all did more or less constructing by many trials to complete a definite recall of the forms of the pieces. This mode of procedure is well illustrated by the following example.

Ex. 14. Obs. *Bk.* ("Recall the first problem you solved.") [10 sec.] "I just saw a visual image of the constructed T, at first. Then I saw two straight lines running from north-east to south-west, and diagonally across the T. I have a distinct visual memory that the notch is in the center piece. On second thought this seems to be incorrect; I do not quite remember the right shape. I have nothing to verify it by. I feel pretty sure that in general it is correct. My process of doubting the correctness of this center piece consists simply in looking at it and in feeling an unsettledness and uncertainty. I try to call up the image of the original, but it does not come. I remember (motor imagery) trying to get them to fit together. I also have visual images of gray pieces." [He drew the constructed T, looked at it, and changed it, making it wider in the stem.]

The main point of interest in the 'ring problem' consists in the fact that every observer first recalled the principal characteristics of the ring and the disc, and then the twist that released the ring from the center hole, all in visual imagery. One might suppose that the twist, the motor act which brought the solution, would be recalled by means of a motor image; but in the case of half of our observers the motor image did not appear in recall; and in the case of those observers for whom it did appear, it did not make its appearance until after the visual image of the act had become clearly established. No tactual images were aroused although, like the following problem, it appealed to the tactual sense.

Though all the observers had worked with the holes that are half way through the disc, only two now remembered that they existed; and this only after all the other characteristics had been recalled. The effort to take the ring off the edge, although it involved a definite motor sensation, was invariably remembered in the form of a visual image of the act of twisting the ring and the feeling of disappointment at the failure. The situation, the room and the surroundings were most clearly recalled.

3. Problem 4. The Touch Problem. (See p. 39 for the statement of this problem.)

In the recall, the first definite remembrance was a visual image of the sheet of paper held up against the forehead; then there appeared tactual images of the earlier experience during the solving. This tactual imagery was so vague that it was not employed in definitizing and furthering the recall of the problem nor had it been so employed in the earlier experience. Visual images of the dots and lines as the observer had drawn them on his paper during the experience came clearly to consciousness immediately afterwards. Then the crux of the problem,—the existence of the two points of view,—was recalled in visual imagery. Note the two illustrations below,—Exs. 15 and 16.

The accuracy of the perception of the direction and distance of one dot from another by the tactual sense proved to be far inferior to that of the visual sense; and after five minutes those tactual impressions which had not been transposed into visual imagery and not written down were incapable of being revived.

Ex. 15. Obs. *Wh.* ("Recall the problem of touch.") [60 sec.] "I had begun to visualize even before you got through speaking, and my attention was concentrated upon this alone.[13] Then I repeated 'touch' again; strain in my forehead, and immediately had a visual image of the paper on my forehead, and tactual imagery of the day's experience. I knew it was the problem. Definite reconstruction of the presentation,—pressure images akin to your first day's presentation came. Then I visualized the drawings I had made, and saw myself making them.[14] . . . I saw an image of the inverted Z that I had drawn because I had not perceived the direction in the first experiment. Experienced again the complex situation. I imagined myself drawing in both directions,—to the left, and to the right. Only now when I have a visual image of myself putting the paper up to my forehead do I visualize the stimulus as it would appear from your point of view. I could not recall it without imaging the whole situation."

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[13] I had a definite set in consciousness to act quickly. This consisted partly in auditory images of your voice, and in repeating the word 'touch' to myself, hearing your words again, seeing myself standing over an iron puzzle of yours [number 4] then the actual tactual perception of the counters of your 'lozenge' problem [number 8]. That is a touch problem, but not the problem you want." [The above illustrates the series of irrelevant images that often precede the recall. The remainder of the introspection exemplifies the points cited in the discussion.]

Ex. 16. Obs. *G.* ("Recall the touch problem") [11 sec.]. "Nothing in consciousness at first except the auditory image of the word 'touch.' . . . Then a visual image of you putting a paper on my forehead, a flash, no readiness, no leading up to it. A visual image of my forehead. Then you put lines in a certain order, and had me draw lines that corresponded to those you had drawn,—vague, visual images of what I drew."

4. Problems 5 and 6. Memorization. (For the statement of these problems see pp. 39 and 40.)

Problems 5 and 6 were presented with a view to bring out individual differences between the various observers in that the memorizing of the numbers would here permit the more abundant use of vocal motor imagery than others of our problems; thus the memory of an individual who is inclined to use the vocal motor in preference to other kinds of imagery, could be compared with his memory for those problems which do not admit of so ready a use of vocal motor imagery. The recalls showed that no one made exclusive use of vocal motor imagery in his revival of the problem; nor were these problems remembered more permanently than any others.

A secondary interest in these two problems has been to see whether the perception of relation would be recalled apart from the numbers between which the relation obtains. Each problem was recalled three times,—at the end of one minute of memorizing, one half hour later, and again one month after memorizing. (During the half-hour interval the observer was strenuously employed throughout the thirty minutes in solving other non-related problems.) In not a single instance during the first two recalls did the remembrance of the relations come to consciousness before the remembrance of the numbers themselves. In other words, the terms were remembered as unrelated data at a stage before the relations came to consciousness. However, the successive appearance of two numbers that had been consciously related was a common occurrence. For instance, in problem 6 the solution involved a consciousness of the fact that 10 is twice as great as 5. In the recall of this problem after half an hour, three consecutive stages were clearly differentiated: (1) a consciousness of a concrete, unrelated 5, succeeded almost immediately by (2) a consciousness of a concrete unrelated 10; (3) and not until the third stage was the observer aware of the relation, namely the fact that 10 is twice 5; and in certain instances the third stage was wholly absent. But in recalling after an interval of a month, the revival of the fact that certain relations had been perceived was first to appear after the visual schema, and while attending to this fact the observer recalled the numbers. However, unrelated numbers were also correctly recalled in numerous instances. The following group of typical examples illustrate, first, the introspective report of the perception; second, the report of the first recall after half an hour; and third, the report of the recall after one month.

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[14] Attention turned back to forehead. Auditory image of your voice saying 'reproduce.' Saw you taking paper away, not letting me see what you had written on your paper against my forehead. The actual images are not plain. The dots, etc., are indefinite." [Pauses 35 sec.] "I have a visual image of the crescent, the Z, the triangle, etc., that you drew on my forehead as I drew them on the paper. I had an auditory image of the noise and of the punching of the pen through the paper." [Pauses 31 sec.] "Now there came and surprised me, a mass of imagery rich in meaning, which aroused much pleasantness. I was amused when I saw through it, and reflected that I could not get it right the moment before.

Ex. 17. Obs. *Fs.* [Introspection upon the act of memorizing problem 6.] "When I picked up the diagram I looked first at 12, then at 27, and found that 27 was not twice 12. I abandoned that and glanced around in search of a number which would be twice 12. An *'Aufgabe'* to hurry, present only in the tendency to look at certain figures quickly. I chose 7 and glanced through to see if there was a number that was twice 7, but I did not get through the whole list. I selected another hurriedly. Not a systematic finishing out of the procedure which as far as it went was somewhat systematic. Not aware of having known definitely what I was looking for, *i. e.,* I did not say, '7' and '14,' and look for 14, etc. I said '5' and then glanced through. My glance happened to fall on 10, and I said, 'Two times five are ten.' The procedure adopted then consisted in glancing through to find numbers that were multiples of three. I hit upon 18, then upon 21 with 7, and finally upon 48. I glanced across at 12, having remembered that it was there. It is interesting to note that I glanced across instead of imaging 12. No other numbers contained three so I repeated the others. . . ."

Ex. 18. Obs. *Fs.* ("Recall what you did half an hour ago.")[15] "First thing was a visual spatial schema localized down here, representing the general outline of the problem. Immediately attention seemed to turn to this side of the schema; I can not now say whether I visualized the figure and schema simultaneously, or the figure first. I wrote 27 on my paper, but there was a period of deliberation as to whether 27 or 12 belonged there, both being present in vocal-motor terms, as though I were speaking the words 'twenty-seven,' and 'twelve' with an inflection of questioning. A definitely localized 19 occurred to me in visual terms, accompanied by vocal-motor; and without hesitation I then wrote 27 and 12 in this form. Then 'three' came and remained as vocal-motor setting in consciousness; a slight tension which did not relax until I had written all the three, 27, 12, and 19. When I set 18 down below, it seemed as though it might belong over above. I thought they belonged approximately in the same line. That is present to consciousness simply as an act of setting it down. The next that came were the 16 and the 5. Then came 8 and 7, and I hesitatingly set them down with question marks. Then came the 10 localized down below. . . ." [The 5 and the 10 were recalled separately and their relation was not recalled.]

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[15] This, of course, refers to the problem of memorizing. The experimenter deliberately presented his instructions in this vague form in order that the clue to recall should be as indefinite as possible.

Ex. 19. Obs. *Fs.* [Second recall, after an interval of one month.] ("Recall the problem of memorizing.") [43 sec.] "At first there was a period of blankness; I was aware of nothing but you and the words you had just spoken, the latter in auditory imagery. Then there came a visual image of a sheet of paper. My attention was primarily directed to the numbers written on it in a vague elliptical shape. I remembered my general method of procedure: reading the numbers hastily without any plan, then catching myself up in the middle and looking for associations, looking at different numbers, bearing one in mind and glancing over the rest to see if any relations could be found. This remembrance consists in visual imagery of glancing at numbers. The auditory imagery (own voice) of the word 'divisible' appeared just now, which meant that I was looking for one number that was exactly divisible by another. Then 18 came; it was visualized down there. Aware that '18' is a multiple of 6,—'6 times 3' (vocal-motor images with dwelling on 6, which I interpret as an awareness that I used 6, in some way connected with 18). [The 18 is correct, but the attaching of the multiple 'three' to 6 to form 18 is incorrect. Note that after the 7 was recalled below, the idea of the multiple three was employed, which finally led to the recall of 21. The 19 came to consciousness unrelated.] Visual image of 7; then said to myself '6 times 3, 6 times 7, 6 times 7, 7 times 3,' which latter gives 21, and this I accepted as correct. A visual image of 19 appeared, then . . ."

From the above examples we see the same general principles that have appeared in all the problems so far treated even though this is a problem in memorization. The visual image is the first definite factor in recall, and is used very freely throughout the process. The recall progresses, by means of a multitude of trial images, from the vague visual image noted above through greater definiteness and detail until the whole problem is reinstated.

5. Problem 7. The Addition Problem. (See page 40 for the statement of this problem.)

For ease of orientation this problem may be introduced by referring back to problem 1. Here also the presentation consisted, in part, in a visual structure; but in the former instance, the general form was that of a square, in the latter, an equilateral triangle. Although in this latter problem, the fact that the numbers appeared in a triangular form had no particular significance, since any other arrangement would have served equally well, yet every observer reproduced a visual schema, in the initial act of his recall. This schema, for all but two observers, consisted of a visual image of vague numbers arranged correctly in the form of an equilateral triangle. This image was clearly recognized as the form which had been observed in the solving; and although the problem could not now be formulated, yet this vague, schematic, visual image signified to every observer that he was in the act of recalling the addition problem.

The two observers, who failed to recall this triangular form of the group of numbers, recalled it as a square which served as the basis of their further recall, and which fulfilled for them in every way the same function as did the triangular form for the other observers.

This problem was more difficult to recall than those just described. It will be noted that the experimenter's cue contained no suggestion of the visual structure of the presentation; and if we may infer that it is necessary first to regain this form (from the fact that every observer recalled the visual schema before proceeding further with the recall), we may offer the explanation that this relatively retarded reproduction was due to the fact that our cue was only slightly suggestive. Several observers were not able to proceed from the first cue,—'the addition problem;' in such cases, after several minutes of non-productive search we gave a second cue: "You were to select certain of the numbers which when added gave the sum required."

In the case of two observers, a third cue was necessary: 'A sum was to be found from a group of numbers.' This aroused a schematic visual image of the form of the group of figures, and a recognition of the fact that at last a recall had been begun. It was one of these observers who recalled the group of figures as being in the form of a square. Several illustrations showing the general trend of recall are here given.

Ex. 20. Obs. *Fr.* ("Recall the addition problem.") [50 sec.] "First there came a visual image of a triangular group of numbers, some of which contained two digits, and others one; no part of the visual image was clear. Then 'Find three that make the sum fifty' came in vocal-motor imagery. . . ."

Ex. 21. Obs. *Y*. ("Recall the addition problem.") [64 sec.] "I cannot recall a thing. I believe that I never had that problem." ("You were to pick out certain numbers which, when added, gave you the sum required.") [The observer repeated these words twice and then said:] "I had a vague, triangular, visual schema; a group of numbers came, but I could not read any of them. Then I recalled myself sitting here selecting three of them or trying to select the proper three. This was mostly visual, too vague to be sure about the three. Now it comes again more clearly, the numbers '10,' '12,' another '5.' Tried many times to make a sum of 25 from three numbers; I got it and went out glad. I see myself leaving the room as if it were only half an hour ago. . . ."

Ex. 22. Obs. *G*. [41 sec.][16] . . . "Now I have a visual image of a triangle of numbers. I now see also faintly the words 'Three numbers that make 25'; also some vague motor imagery of picking out three numbers and of running my pencil around over the group. I took one number and added two others to it; I see myself as I did this. . . ."

In the last two examples it is to be noted that the number recalled as the sum to be found was 25 instead of the correct number, 50. This same substitution occurred in the case of three other observers. It may have been due to the fact that in the search for the sum 50, 25 was the first to be tried as one of the numbers that make the sum. A 15 was also seen and then a 10 was looked for, to complete the total sum of 50. No 10 was to be found in the group, so both the 25 and the 15 were abandoned. When at last the 19 and the 6 were selected and added, giving a sum of 25, the problem was practically solved, for the observers remembered visually that the group contained a 25 at the left; they did not need to turn the eye to it, nor to add it in to form the sum 50. That the 25 was there was sufficient for these four observers; and 25, instead of 50, had become the goal. The fact that the number 25 was ideated both at an earlier and at a later stage as the sum of the 19 and the 6, proved to be so impressive that, in the recall, 25, instead of 50 was taken to be the goal of the problem and the sum to be found.

Three other observers pursued the same general route; but they again found the 25 and added the three numbers, making 50, the sum required. It is to be noted, in contrast to the recall by the four observers who did not complete the sum 50, that these three recalled the 50. Two observers found the 25 and the 6,—which gave a total of 31,—and then found the 19; but they did not add the three numbers after the 19 was found. Consequently, for these two observers the first number to appear in recall, two months later, was a visual image of 19. From this they proceeded to the recall of the visual image of a typewritten 50, as presented to them in the original statement of the problem. One observer, who carelessly found the correct three numbers that made a sum of 50 by simply choosing the numbers and not adding them to see whether they made the required sum, failed to recall a single factor in the problem except the triangular shape. He recalled the numbers 100 and 55 of another problem as belonging to this one. When told he was wrong, he immediately recalled 25 as one of the numbers employed to form the sum (in definite visual and vocal motor imagery).

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[16] "At first I was conscious only of your words, 'addition problem' in auditory imagery; then a visual image of 152, 3, then a column of figures, but these were imagined. not remembered. I remembered that the sum of certain numbers should be 40, but was aware that this was another problem."

From these facts we may conclude that the goal or final step taken in the solving is most likely to be recalled first, and that this may then be transposed to stand for the statement of the problem.

To continue our comparison of the recall of this problem with that of the 'dot problem,' we may state that not only did the general form (visual schema) appear first and seem to be most important because it constituted the starting-point of both problems, but also that many trial images which had been employed during the first solving of both were never recalled. The first trial image which had been employed during the original sitting in the search for the solution was frequently recalled, but practically no other members of the original series. The contents that were most subject to recall were the visual form of the group, the successful trials, and the relative spatial position of the numbers employed in these trials.

For seven of our ten observers, the relative positions of the three numbers in the visual schema were more definite in recall than were the numbers themselves. Even before the numbers appeared, the observer clearly recalled the fact that the largest of the three had been the last used in forming the sum. When the numbers cleared up they were all in their proper places. The act of recalling them was very similar to a difficult visual observation where a searching or a peering into a dimly lighted region brings indistinct objects more clearly into view.

In their search for the proper numbers, several observers began systematically with the top figure of the schema and combined every possible pair of numbers with it to see if they yielded the desired sum. Then the number next to it was treated with every other two, and so on, until the solution was reached. It was found that all observers remembered this procedure very clearly, in terms of motor or visual imagery; but the numbers themselves were not recalled until later. One observer, instead of searching for three numbers whose sum is 50, adopted the more rapid method of finding three numbers the sum of whose right-hand digits is a multiple of 10. This method yielded four combinations of numbers whose sums ended in zeros; but each of the first three combinations gave a sum of 60. The fact that this method had been employed was remembered in the subsequent recall, but with certain errors of detail,—a 5 was substituted for the zero at the right.

One might, of course, expect that in arousing a series of trial images in recall a process of reasoning would be employed. As for example, when one number is relatively stable in imagery, the other two which are brought up as trials shall complete the sum 50. This is shown very clearly in the following examples.

Ex. 23. Obs. *F.* [18 sec.] "At the outset I was conscious of nothing but the pyramid which was present as a vague visual image. Then came 19, and I immediately saw that 31 must be added to it to make 50." . . . "It seems that I was to find *three* numbers whose sum is 50. I recall the act of searching for three numbers in the group,—a partly motor and partly visual consciousness."

Ex. 24. Obs. *W.* ". . . . I had a 25 and a 10, in auditory, vocal-motor, and visual imagery, then a vocal-motor 15; 9 was localized visually near the bottom and to the left. Here I supplied a 6, in auditory imagery, and tried to localize it visually above and to the left of the 9.[17] ["The 9, in fact, was a part of the 19 of the number employed in forming the sum during the solving. The 6 visualized above and to the left had also been employed in the original solving. In an effort to form the 25 as the sum to be found, the observer in recall employed the trial images 8 and 7 but they did not persist. Later on the observer found the correct three numbers that form the sum.]

We find that all ten observers reproduced the visual form of the arrangement of the numbers, which grew clearer with the lapse of time during the act of recall. For five observers the position of the three chosen numbers then appeared, and later, some of the other numbers, especially the three that made a total of 50. Four observers recalled the appropriate numbers correctly,—25, 19, 6; and four recalled the 3 which had occupied a conspicuous position at the apex of the triangular group of numbers. Three others spontaneously reproduced the 19 without recognizing that it was being recalled; two reproduced the 6, and two the 25 in a similar manner. Every observer when urged by the experimenter to guess or create if necessary in order to complete the formulation of the problem (which by the way demands a consciousness of the solution), correctly reproduced the three numbers which in the original formulation make the required sum; but they also reproduced more than half of the other numbers in the group. These facts are interesting in that they exemplify so clearly the predisposition, of the individual to follow a beaten path although he is confident meanwhile that he has struck out upon a new path, *i.e.*, he feels that he is 'guessing' and creating a wholly novel product. In many instances, however, certain of these imagined numbers seem to become endowed with a slight feeling of familiarity, and they then seem to the observer to be correct remembrances.

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[17] These come again and again but with only a moderate degree of certainty; 8 and 7 come in but they do not persist and I am much less certain about them. I was in doubt as to whether 25 or 50 was to be the sum. This, I believe, was due to the fact that visual images of both of these numbers appeared, and attention shifted from one to the other, several times; this has left me in a state of doubt. . . ."

In the recall of this problem, the trial procedure proved to be the most efficient method. The observer who evokes many visual, vocal motor, or auditory images, and who then proceeds to test them is most successful in attaining a complete and accurate recall.

6. Problem 10. The Weight Problem. (For a statement of this problem see page 41.)

This problem, like problems 1, 4 and 7, will be treated in detail. Those lying near it in the series are so similar to it that they need not be discussed separately. For example, problem 8 bears a close resemblance to this one, both in its complexity of statement and in the number of short-cuts that may be employed. Problems 9, 11, 12, 13, 14, and 15 are all concerned with concrete materials. Since they draw upon various kinds of experience one would expect that certain observers would react more promptly to one problem than to another, inasmuch as their past experiences differ. However the following description of procedure with the weight problem is typical of more than a dozen of our problems.

In order to show the detailed nature of the recall we introduce here a few facts of the first solving,—the facts that are to be recalled. Seven observers failed to perceive that this problem really hinges upon the question as to what sort of weighing apparatus was to be employed. They simply proceeded to divide and to sub-divide the weight,—assuming that an ordinary balance was to be employed. The other two observers promptly responded that to balance any number of pounds from 1 to 40 would require an infinite number of weights. This is correct, unless we consider that "any number of pounds from 1 to 40" refers to integral numbers alone. These two observers were instructed to restrict their interpretation of the problem to whole numbers of pounds, and to proceed with the solving on that basis. In their subsequent recall they both failed to remember that they had had any difficulty with this point.

Every observer reported as his first solution, forty one-pound pieces. In only one instance, however, did this tentative solution appear in any way in the recall; and for this observer it was simply a bit of relatively non-significant imagery in the course of his interpretation. Five observers saw the necessity of adopting a certain kind of scale for weighing, in that each had a visual image of a balance, with its weights; and it seemed to them to be the only possible means of solving the problem.[18] These images of the weights for the balance were of appropriate sizes,—each one of a lower denomination being just half the size of that of the denomination next above it, except in the case of the three weights below five grams which are two, two and one.[19] This envisagement led these five observers to consciously imitate this method of division, and it possibly influenced the other three, who did not have the envisagement clearly in mind, to use this method. These envisagements were in every case vividly recalled.

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[18] No one reasoned far enough to see that one weight is sufficient to conform to the conditions of the problem. That is, if we conceive a balance-arm on which this weight may be shifted so that it is at any distance from the fulcrum, any weight up to forty pounds can be weighed with the undivided forty-pound weight.

[19] A frequent error in solving was that instead of correctly dividing the forty pound weight, first into two twenties, then one of the twenties into two tens, then one ten into two fives, and finally one five into two twos and a one-pound weight, the observer divided the last five-pound weight into a three, a two and a one. This error occurred in the case of seven of our ten observers, each of whom corrected it, however, during the solving. It reappeared for five observers, two of whom recalled that this had been an error in the act of solving, while the other three traveled the old route, made the error, then corrected it, but failed to recall that they had made this error before. We might infer from this result that the probability of the recurrence of this error was very great as compared with other errors. Or we might with greater assurance infer that the appearance of this error in the recall in many of our cases was due to the fact that the reappearance possessed rather the character of re-solving than of pure recall. The lighter weights could be either two ones and a three-pound, or one one-pound and two two-pounds. Nine observers employed the latter division; only one used the former, in his solution of the problem. Every observer in his recall revived the grouping he had formerly employed.

Five observers instead of dividing the weight into halves, quarters, eighths, etc., began to build up the numbers from the one-pound weight that was first seen to be necessary, then to adopt a two-pound (which, combined with the one-pound weight, would weigh three pounds); then to choose a four-pound weight (which, combined with the one- and the two-pound weights, would weigh any integral number of pounds up to eight pounds), and then to choose an eight-pound weight, etc. They did not at first attend to the fact that the sum of their weights must equal forty pounds since that is the weight which is to be divided; but they finally corrected the error. These five observers recalled their method in the visual images of the figures which they had written on the paper, such as 1, 2, 4, 8, etc., as is shown by examples 25, and 28, pp. 67 and 68.

In the procedure of the five observers just described the process of adding weights of one or two pounds to obtain weights of greater magnitude was carried on until ten was reached. Then they saw that since by means of the one-, two- and four-pound weights, any number of pounds could be weighed from one to seven inclusive, and since with the eight-pound weight any number of pounds up to eight plus seven could be weighed, they need not count out the particular weights that are necessary to form each integral number of pounds up to the point where all the weights are placed on the balance and the sum fifteen is found; but they saw at a glance that all up to sixteen pounds could be weighed, whereupon they chose sixteen pounds as the next heavier weight. Three of the five observers who generalized in this manner, recalled more or less accurately the group of numbers that they had found to be the weights desired; and these visual images signified to them that they had begun with the one-pound and had tried each number of pounds, but in no case were these trials recalled. We should here emphasize the fact that visual imagery constituted the structure of a recall which served as a generalization. This has not been an uncommon occurrence in our results with other problems, as was pointed out earlier (p. 52). The few isolated images often signify a complete course of mental processes that had taken place during the solving. For illustrations of this, see examples 4, 11, and 12, also 25 given below.

Ex. 25. Obs. *B.* [Recall of the weight problem.] ". . . I had a visual image of my writing on paper, my former act of solving the problem. Then I saw a column of numbers,—1, 2, 3, 4, 5, etc.; then it occurred to me in vocal motor imagery, 'I now remember what it was about.' Then I set to work to solve it, choosing one pound, two pounds; did not need three because I already had one and two . . .[20] Alway come back to the solving. Four pounds? Yes, I must take that. I do not need five, nor six, because I have four and two; do not need seven—four, two and one. Eight? I need eight, but do not need 10, nor any other up to 16. . . ."

The above introspection indicates that a visual image informed the observer of his former method of solving, which, in turn evoked a remembrance of the statement of the problem and a tendency to resolve it. It is to be noted that the 'short cut,'—the last step in the example given above,—was made in this recall after he had tested the numbers up to eight, while in the first solving he did not generalize until after he had tried every number up to twenty. This 'short cut' in recall was not recognized as the same kind of step that had been discovered during the solving, as his further introspection shows.

Ex. 26. Obs. *B.* ". . . As I looked over the paper at the sitting I found a short cut, which simplified the process of solving it; but I can not find it now. . . ."

The first factor to appear in the recall of this problem, was a visual image of the balance, of the weight (iron), or of the written solution of the problem. In one case the lump of coal (weight) that had been imaged in the solving was the first datum to reappear. Another observer recalled first the diagram that had stood for the solution; eight observers reported that the visual image of the balance appeared first. These images of concrete objects, which the observers had employed during their solving of this problem, now signified to them that they were on the right track, that they had begun to recall the desired problem. These images possessed a definite meaning, which, when attention was directed to them, developed into, or initiated further processes. These processes were very similar to those that are illustrated in Exs. 5, 6, 7, 8, 9, in problem 1. This initial image was clear and stable from the outset; and this fact afforded a feeling of certainty that it was a recall, as is illustrated by the following examples.

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[20] At this point came a feeling of doubt. I wondered if I was limited in the number of parts or pieces into which the weight was to be divided. I believe that this consciousness consisted in vocal motor imagery of, 'how many parts is the weight to be divided into?' plus a feeling of pleasantness.

Ex. 27. Obs. *Fs.* [20 sec.] "At first there was an absolute blank; no trace of familiarity; then the word 'divide' vocal motor, and auditory imagery, occurred to me together with a visual image of a cylindrical mass of iron on this white sheet of paper. The cylinder seemed to fall apart into divisions,—20, 10, 5, etc., were present in visual figures. That seemed to involve a consciousness that the weight had been divided into parts,—the latter coming with a good deal of conviction. The essence of that consists in the clearness with which that image appeared,—the whole image was somewhat reduced in size. I was aware of your sitting here, and remembered the awareness of the former situation. Those features were very clear in connection with the certainty that I had begun to recall the correct problem."

Ex. 28. Obs. *Wh.* [5 sec.] "The main points in the problem came out immediately. The word 'weight' appeared in terms of a visual image, exactly the same as I saw at the first sitting. There were the weights used on the scales at home. Recurrences of the visual images I had at the time when I was solving this problem?.[21] I had visual images of the paper I had used in the solution which meant to me that I had been given the problem to divide the weights into one-pound weights, to produce forty one-pound weights."

The following introspections illustrate the extreme activity which sometimes characterizes the act of recall.

Ex. 29. Obs. *B.* ("Recall the weight problem.") [4 sec.] "Farmer and hogs?" ("Introspect.") "Visual image of typically dressed English farmer driving pigs down the road; then a visual image of a typewritten sheet of paper, but could not quite read it. Then, in vocal motor imagery, 'If the hog had weighed twice as much,' repeated several times, but got no farther." [Here, B had evidently been recalling the hog problem.] ("A certain weight to be broken up.") [3 sec.] "Oh, yes. Visual image immediately of a set of weights, for a chemical balance, fitted in a block of wood, silver white, of different sizes. Saw distinctly at last. Then it occurred to me that some object had to be weighed; and that the weights were not of the right size to do the weighing. Even then I was in doubt about details. I think the problem was to take a certain amount of material and weigh it in lots of uniform weight. The structure of this consciousness was a visual image of a druggist who was in the act of weighing, putting up powders in papers, all of equal weight. A feeling of pleasantness and satisfaction that I had succeeded in recalling it. Then I felt that it was another problem I found in the last week. A *Bewusstseinslage* of despair. Had an image of a mass of something that looked like stone, to be weighed, but that flitted past. Then I saw a chemist behind a counter weighing something. The chemical balances were on the counter. This train of images was fleeting and vague. A feeling as of groping for more images to help me solve the problem. . . ."

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[21] Saw the weight cut up into different pieces,—cut up into different bundles. Visual image of the word "cut" and a visual image of the figure "40." Then I had a visual image of the problem typewritten on a sheet of paper. Then I saw this much of the statement of the problem "cut up the weights into 40 different pieces," but I did not get any farther.

Ex. 30. Obs. *R.* [3 min.] "I imaged a lump broken into pieces as the other day, then a vocal motor image of 'forty-pound lump.' I almost said 'coal,' but realized it was not coal. The forty pounds to be broken into such pieces that . . . I can't get it. I had a feeling that it was right so far. Rather believe that I failed to solve this problem. This I infer from the feeling of vagueness." [Thinks 2 minutes.] "Many irrelevant images appear; I visualized a pair of scales, visualized '5 to 1,' '3 to 5,' '8 to 5,' '4,' '3,' '2,' etc., meanwhile looking for something which might seem familiar. 'Broken into such parts' came vocal motor. I saw several persons. 'John and Henry' might be given parts. All discarded, not familiar enough. . . . I believe that the problem dealt with an arithmetical progression, which came in the form of a visual image,—a series of digits dimly visualized, '2, 3, 4, 5, 6,' etc." [He had actually set down these figures during the solving.]

C. *Main Points and Generalizations from the Recalls of all the Problems.*

The question which has concerned us in this investigation may be stated most comprehensively as follows: What mental processes recur in the recall of problems that have been solved but once? We shall here treat these factors separately as nearly as possible in the order of their appearance in the act of recall; and we shall also indicate the relative significance of each of these factors for the act of recall.

1. *Time Required, and Cues Necessary to evoke Recall.* The time necessary for the initial act of recall varied widely for different observers, from almost immediacy to several minutes. If, after an interval of a few minutes, the observer had not obtained even a feeling of familiarity with the object to be recalled, he was given a more specific cue, after which, if still unsuccessful in evoking recall or indications of it, a second cue was presented. In general, those problems which had furnished the more abundant and the more vivid experiences during solving were most readily recalled. The vividness of an experience during the solving may be dependent upon the novelty of the problem, and novelty may therefore be the main cause of ease and rapidity in recall. For example, the 'box problem' was a very ordinary, 'every-day' problem, and it was very slowly recalled by every observer; but the 'spider problem,' which dealt with a strikingly novel situation, was recalled with great rapidity. Those problems in which a physical object, a picture, or a diagram was either presented or imaged, were most easily revived. Problems of the opposite nature such as those of 'war,' 'propositions,' 'linnets,' and 'cramming' were always slow to be recalled; and they always needed many cues to start the recall. In only eleven cases was it necessary to re-present the whole

problem before the observer was able to begin. In such cases he recalled very little of the procedure which he had adopted during the solving. In fact, in several of these cases, the route which had been followed in solving was pursued in the act of recalling but was not recognized.

About half of our recalls were evoked by means of the general instructions: "Recall the first problem you solved;" "Recall another of the problems solved early in our series," etc. We had selected several stereotyped forms for presenting our cues, from which we never departed. Half of these were of a concrete nature and half were abstract. We have made a census of the degree of success or failure which attended the use of each type of cue, and we find no essential difference of evocative value between the two. The abstract cue afforded the observer a wider range of materials to recall and hence our quantitative data are of little value. But we are able to state that the course of recall was very similar in the two cases, except that the abstract cue often aroused several trial images that varied widely, whereas the concrete cue often called forth trial images which varied within narrower bounds. An examination of the introspections which are cited above, shows the results which follow from the employment of various concrete and abstract cues. The employment of a concrete cue should of course be compared with that of an abstract cue in the case of the same observer, as, for example, Exs. 16 and 31.

2. *Attitude and Procedure during Recall.* In the initial stage of a recall, observers vary somewhat widely in their customary attitudes. One observer is very deliberate in his search for the parts of the experience he is attempting to recall. When an image appears, he attends to it very carefully and is in no haste to arouse other images until he has examined this one in detail. During this act he usually discovers many associated images which are present in vague form. He attends to them in a calm and deliberate manner, proceeding in this fashion until finally he has recalled the desired experience. Other observers are very uneasy and struggle very impatiently for a few seconds when one or many images appear which arouse still greater excitement. Their goal is to evoke the problem in all its details at once; and it often happens that many of the images are passed over slightingly, which, if attended to more carefully, would undoubtedly establish a firmer route of recall.

It is very interesting to note the many goals which the observer constructs for his procedure in the various recalls.

The deliberate observer mentioned above sets about very often to find a significance for a single unimportant datum that appears, and in so doing wastes his time. The hurried observer is apt to rush on beyond some important images and lose himself in minor details, consequently is unable to formulate the problem or its solution without reviewing and reorganizing his objects of recall. The following instances show typical attitudes during recall: Exs. 10, 14, 24, 27, 30, 31. (Read in this connection the description of the characteristics of the appearance of the images of recall, p. 72.)

In most cases of recalling after a month's interval there is first a period of searching which continues for only a few seconds, and is immediately followed by a more or less profuse succession of images. When this second stage is reached the observer has usually ceased his search; images now flood in upon him. His attention is commanded by them, rather than they by his attention, as was the case in the first stage mentioned. Many deviations are found, but in a majority of cases the above two stages are present.

3. *The Structure of Recalled Experiences.* a. The Feeling of Familiarity. As has been shown above in the statement of the results of the individual problems there sometimes occurs a feeling of familiarity, a feeling that the earlier experience is just about to be revived. This is not an unusual phenomenon in every-day life; its significance for recall seems to consist, for the most part, in the fact that it gives encouragement and incites one to grope and search for the datum which is almost within one's reach. The number of attempts to recall, in which this feeling of familiarity preceded, is relatively very small in our experiment. Exs. 3 and 4 show the general characteristics of such an experience.

b. The Image. In ninety-five per cent. of our recalls a visual image was the first definite factor to appear. This characteristic may be seen to be prominent by glancing over the introspections quoted above. It is surprising to discover that both the solving and the recalling of such a variety of problems is mediated by visual imagery. Although tactual, temperature, kinaesthetic, auditory and vocal motor images had been employed in the solving, many of the problems were recalled in terms of visual imagery, such as, images of one's self writing, of sitting in a thoughtful posture, etc. (See Ex. 9, p. 51, Exs. 15, 16, pp 56f.) However, in the cases of those problems which had to do less with concrete objects than with turns of language or with hidden meanings or sentiments, verbal imagery,—auditory, vocal motor, and visual,—appeared more often than elsewhere.

On account of lack of space we do not include a table showing quantitative data regarding the various kinds of imagery that appear in recall, for each of the three modes of initial presentation of the problems that we employed. This correlation is of little value, because in the solving of our problems the mental processes are far more numerous and more significant to the progress in the solution than is the mode of presentation. In only a third of the cases of recall did the observer evoke a remembrance of the mode of presentation, visual (print or script), or auditory; and this remembrance was usually the reappearance of a single word, or visual form, as *e.g.*, the auditory word 'six,' or the typewritten numbers 6, 6, 4, or the circles arranged in the form of a square, as in problem 1.

c. Characteristics of the Appearance of the Images of Recall. In a great majority of the recalls, the images first appeared in a rather vague and schematic form, no matter to what modality they belonged. Tactual remembrances appeared in a series of not definitely localizable images of tactual impressions upon the forehead. The kinaesthetic imagery of moving the lines in the 'dot problem' (see Ex. 9, p. 51, and p. 56, the 'ring problem'), never became clear and definite. Vocal-motor and auditory images often came in a vague and disconnected manner. Observer *W*, who reports more auditory memory than any other observer, often images words but these so indefinitely that for some time they cannot be spoken nor can their meaning be given. Many examples of our recalls, which for want of space we cannot include here, show that although the images first appear very vaguely, they gradually develop, and become definite and meaningful. (See Exs. 22 and 25, pp. 61 and 67, Ex. 27, p. 68, etc.)

The images that come readily and are clear at their first appearance are usually endowed with particular meaning. For example, when the observer without any other clue than "Recall another problem" had a clear visual image of a farmer driving hogs down a country road, or of a milkman with a number of milk cans, or of a schematic cross-section of the earth, he had at the same time a meaning for each. A farmer driving hogs has almost always been associated with going to market; this association was therefore immediately reinstated, and the image was endowed with meaning,—a problem about a farmer taking hogs to market. Then came the association (not a new one at all) that hogs had been sold, and that the problem had to do with the weight or the price. Now the two questions which are the most usual ones in every sale of hogs are: What is their weight? and what is the price per pound? The word weight as it occurred in a visual, an auditory, or a vocal-motor image, or was actually spoken, was very familiar. He remembered that the problem was about weight; and, at this point, he saw his diagram or his algebraic solution that he had made during the solving; or the vocal-motor, auditory, or visual image "half" was recalled. The problem and its solution were now almost completely revived. A few other words or symbols appeared,—such as, 'score,' '7 score,' '140,' '280,' or 'half its weight,'—and these soon found their proper setting, and this completed the recall. Such an act of recall was often accomplished in a few seconds. In certain cases, however, the processes of recall continued for several minutes.

A very clear example of growth in definiteness, in detail and in meaning is found in every observer's recall of the 'density' or 'boat problem.' When the cue "Recall the density problem" was given, there appeared either the visual image of the boat (problem 22) that they had imaged in solving this problem, or a schematic visual image of a cross-section of the earth (problem 24). Whichever came was satisfactory and seemed to the observer to be appropriate. For instance, he visualized the boat passing from fresh to salt water, and rising just as it did in the initial apprehension of the problem. Several observers stated the problem at this point, by translating their visual imagery into words: 'If a boat passes from fresh to salt water, will it rise or sink in the water?' But they did not rest here, for this statement seemed too simple when it had been expressed and completely envisaged. Immediately afterwards there appeared a visual image of the water-line, or a visual, auditory or vocal-motor image of the word 'water-line.' This signified that the problem had to do with the water-line; hence they stated the question in some such manner as this: 'Will the water-line rise or fall when a boat passes from fresh to salt water?' Hereupon they visualized the two water-lines,—first, the line of intersection of the surface of the fresh water with the boat, and second, that of the boat with salt water. In these images they perceived the answer that 'The water-line falls.' No one remembered the answer in a purely verbal form. They all recalled it in imagery of a concrete visual sort. Every observer first recalled the solution of the previous day; six recalled no more; while four saw again the old predicament that had baffled them, the predicament that after they had attended to the visual image of a fixed water-line painted on the hull, they conceived a second meaning of water-line,—the movable line of contact of the water upon the hull (a series of visual images of the boat showing the shifting line). This movable line they saw lower on the side of the boat with the rise of the boat; and if that be the correct meaning of water-line, then the answer is that the water-line falls. The meaning here was contained in the images. Without them there was no meaning; and beyond them and the context associated with them no observer could report any meaning. This whole experience was as clearly a revival of the original perception as can be found. A typical example of the development of images and meaning is given below.

Ex. 31. Obs. *G.* ("Recall the density problem.") [14 sec.] "I repeated the word; then had a visual image of an oil can and of the word oil, but dismissed them. Then one of oil and water in the can. This was all over before I could shut them off. Then a vague visual image of a ship, on the ocean. Then a question, in vocal-motor terms: 'Would it float higher on salt water than on fresh?' A visual image of the ship rising as it went into salt water. I was then aware that I was recalling the right problem. . . ." "I saw water-line on the ship going up with the ship. Then I saw the water-line going down while the ship rose. Here I had a faint auditory image of myself asking you, during the former sitting, 'What is meant by water-line,—its definition?' I now had a meaning of water-line, in visual imagery from the beginning. I saw first the ship going up and I just saw the line on the boat going up. Then I looked at the water-line going down, *i. e.,* the contact of the surface of the water on the hull going down as the boat rose. This was the final meaning it seems; it remains stable."

The above introspection shows clearly the course of development from a vague schematic image to one which is rich in meaning; it illustrates the trial images which occur in the beginning before the accepted recalled image appeared; and it shows that the meaning is bound up with or contained in the images. The permanence of attention upon the last-mentioned of the images signified its correctness. This is a typical example of the attitude of certainty which is induced by stability, by clearness of the image, or by lack of other images; this attitude is frequently allied with feelings of satisfaction, and the like. It is interesting to note that no characteristic difference exists between the certainty which occurs in recall and that which occurs in the first solving. (For illustrations of this see Exs. 10, 13, 14, 24, 27, 31.)

As has been shown in Exs. 12, 15, 18, 24, 25, 30, 31, which are typical of the other problems, there has been much trial imagery. Many gaps have been filled in by a process which is in nature very similar to creative imagination. In accordance with our definition (p. 46), we have in many instances, called this process reasoning. The observer often infers that more of the problem is yet to be recalled, because the bits of experience that he has revived constitute neither a problem nor its solution; or because what he has recalled may appear to be much more simple than any other problem that he remembers. Consequently he constructs and fills in the parts necessary to formulate a probable problem. He often writes down this material so that he may test its familiarity to the eye; he may say it over to test it by the ear, or by its vocal-motor 'feel' (the 'feeling' of saying it). These aids often bring the recognition or rejection very soon; and in many instances they promote further recall. (See Exs. 12, 19, etc.; also see discussion, pp. 50 and 73.)

Throughout the various problems that we have treated in more or less detail we have, here and there, seen examples of an abundance and even a profusion of images (Exs. 6, 7, 8, pp. 50f.; 12, p. 53; 15, p. 56; 22, p. 61; 24, p. 63). Often during a few seconds of recall, many more images had been present than could be stated. At times every observer exhibited this characteristic; however, it was more common for some than for others. It seemed to depend, in a great measure, upon the condition of the observer. One who is least likely to be excited under trying conditions and who always deliberates, seldom found so many rapidly fleeting images that he was unable to describe them. Several of our observers who are readily aroused or easily disturbed in ordinary activities often become overwhelmed with a multitude of fleeting images. These may be not only of one sensory modality but of several at the same time. Ex. 6, p. 50, Ex. 24, p. 63, Ex. 30, p. 69, as well as the following illustrate this point.

Ex. 32. Obs. *Hn.* ('Recall the ring problem.') [22 sec.] "I had a clear visual image of the ring,—the motor twist of the solution, the methods of trying, etc.,—all about equally vivid and simultaneous."

Ex. 33. Obs. *M.* ("Recall the problem next in order.") "I recall the whole experience, images of this room and of the general situation. It was a dark, warm and cloudy day and the hall leading to this room was dimly lighted. I saw the pieces of paper and myself drawing the diagrams. These came in exceedingly rapid succession. . . ."

If an observer had remembered very little of an experience, his recall was in most cases extremely slow and his images were faint and few. (Exs. 3 and 4.) This is in contrast to the recall of an experience which had been almost entirely remembered, in which case the images were many, vivid, and stable, as Ex. 32 shows.

Let us summarize here the essential characteristics of the recall. When one is able to recall a series of data which corroborate one another, feelings of familiarity, of certainty and of reality are aroused, although the images be in some cases fleeting and vague. The observer then 'knows that he knows.' In other cases a few very vivid images also afford a high degree of certainty to the observer. Such images are recurrent or stable; and if they do not conflict with one another, they signify to the observer that his recall is correct. No single datum of recall is sufficient to enable an observer to say that a given mental experience is a recall. The series of corroborative images (or the context) which gives rise to feelings of familiarity, and the like, is the prime factor in distinguishing an experience as one of recall.

d. Affective Processes. A number of our problems were worded in such a manner that the observer became suspicious of a 'catch' and attacked the solution only after a very careful reading. He often felt that he should be able to solve a problem creditably, yet he could not do so for some time. Consequently various 'feelings' of doubt, of uncertainty regarding his own ability to solve the problem, of chagrin and even of despair, arose before the dawn of successful operations. These were frequently followed by 'feelings' of buoyancy, triumph and satisfaction. In only a very few instances did the observer, after a month or more, recall that any of these feelings had been present. Intensive feelings of chagrin, which had arisen when the observer was reproved for disobeying instructions, were never recalled even though the other components of the experience were readily reinstated. None of our three score recalls of affective processes were evoked except after or together with images of parts of the former experience. We infer that they are of relatively little moment in the act of recall. Ex. 21, p. 61, and the following examples show the recall of affective processes connected with imagery.

Ex. 34. Obs. *Wld.* ("Recall the second problem.") [This is the 'train problem.' 25 sec.] "I believe that the cars and shunting came second. I had a visual image of a T, then the whole situation, where I sat, where you sat, how I solved the train problem, the diagram, all very vaguely. A real track was seen in visual imagery. As I described it, I became more certain that it was the next problem after the T; and there came a faint remembrance of the feeling of novelty that I had had at the time of solving."

Ex. 35. *Ck.* [Recall of the Uncle and Nephew problem.] ". . . I involuntarily began to reconstruct the scene and at the close of this visual recall of the room, the day, and the problem, I recalled the original satisfaction that had attended the solving. This clear, affective toning was a part of the recall of the earlier experience. . . ."

For some observers various attitudes of helplessness, discouragement, and despair frequently arise during the initial stage of recall when the experience had been almost completely forgotten. These are not common, nor is there any regularity in their appearance.

e. The Recall of Processes of Reasoning. 'Short cuts' during the solving were discovered by every observer, but they were seldom recalled. In introspection 25 the 'short cut' which had been made during the solving was hit upon at an earlier stage in the re-solving; but it was not recognized as the step which had been taken during the initial solving. Ex. 12 and 13 also show that the processes which we term reasoning and which had been present during the solving were not recalled. The recall of 'short cuts' is best illustrated in Ex. 28, and in the counter problem, number 8. Nearly every observer reported a visual remembrance of the act of moving a series of counters by means of a single move of the hand; but they counted it as many moves, because many counters had been included in the single movement. The visual image of the pile of counters, of the hand shifting them by a single movement, and of the numbers were the contents of this remembrance of the 'short cut.'

Resumé and Critique

The eighteen individuals who served as observers were drawn from China, from Japan and from widely separated regions of the United States and Canada. The striking similarity in mental content and in mental procedure which our investigation has revealed can scarcely be referred to similarities of social or educational environment and training. Our results differ widely from those of the German and French investigators who report the discovery of non-imaginal elements that stand alone in consciousness; we have found that no datum was ever recalled without images. These images, however, frequently embodied very intricate features. For example, the image of the weights which constituted the solution in the weight problem, of the water-line in the schematic image of the side of the boat, which constituted the crux of problem 22, and of the path followed by the spider (problem 12) were rich in meaning, and they furnished the most essential logical relations of these problems. Our results do not show a single remembrance of relations,—such as perpendicularity, contrast, proximity, etc.,—except in connection with some sort of imagery, vague and schematic though it frequently was.

Michotte and Ransy confined their investigation to the remembrance of related words and phrases. Their observers did not report what relations had been perceived during the initial observation; and consequently they could not guarantee the recall of any, although they reported the recall of many relations as non-imaginal elements in consciousness. The cue which these investigators employed to initiate the act of recall was one of the words of the pair between which the relation was supposed to have been perceived during the initial observation; and for this reason it seems probable that the alleged recalls of relations were perceptions of relations within the perception or the conception of the stimulus-word which was given to evoke recall. Hence their findings do not seem to justify the statement that one may recall relations which are the sole content of consciousness, and which are, therefore, elemental in nature. If the observers had been asked to recall the whole experience without obtaining a single cue, undoubtedly their report that recalled relations stand alone in consciousness or exist only in connection with imaginal components would have been more convincing.

Bühler reports that his investigation also reveals the existence of a consciousness of relation wherein no imaginal content is present. His investigation is clearly open to the same criticism that we have urged against the above investigation. Schwiete found that feelings of familiarity constitute the sole structure of certain concepts for some observers. But here again one has no guarantee that the concept which he is attempting to ideate is completely aroused apart from the perception of his stimulus-word. The feelings of familiarity that he found may be very like those that we have noted in the stage of recall which precedes the appearance of definite features. A second essential difference between Schwiete's investigation and our own consists in the fact that his materials were more abstract than ours, since he employed general ideas and relations that had been habitually perceived while those that we employed had been perceived only once or a few times at most; because of this fact one might expect that Schwiete's method would arouse less concrete contents than our own experiments revealed.

Several investigators have mentioned the predominance of the visual image in recall; but none have found so high a percentage of the components to be visual imagery as our results indicate. Our results also show that vague and schematic visual imagery is very frequent, especially during the earlier stage of the act of recall. The cause of this extreme frequency of visual imagery is not apparent from the present investigation; but it seems probable that the extreme unfamiliarity and novelty of a majority of our problems was effective in producing this condition. Our procedure demanded a very thorough ideation of the data of recall because our problems were rather complicated and involve very delicate shades of meaning; we may infer that this more complete ideation may account in part for the striking predominance of visual images in the present investigation.

V. Summary

1. The act of voluntary recall is usually characterized by two stages. The initial stage consists of an *Aufgabe* to recall and an act of 'searching.' It may continue for only an instant or for several minutes. When the desired datum is readily recalled, the consciousness of activity may be of slight intensity; in extreme instances it is wholly absent. But in cases of difficult or hesitant recall, the act of searching may be conscious, voluntary and long-continued. During this initial stage the observer adopts either a relatively active attitude and procedure, which consists in a vigorous groping and an attentive searching for missing data, often calling up all sorts of images and reviewing them to determine whether or not they are the missing data or hoping that they may cause the arousal of the desired past experience, or a relatively passive attitude and procedure which consists in calmly, but expectantly awaiting the images of recall. Either procedure may be carried through in an unsystematic fashion,—as a planless examination of images which are already present to consciousness, and an equally planless groping for images which seem to lie just beyond one's reach; but in numerous instances, the observer proceeds in a deliberate, methodical and systematic fashion. Occasionally during the long continued search, definite and intensive *Bewusstseinslagen* of satisfaction, buoyancy, doubt, helplessness, despair, or others, enter consciousness. Their arousal is apparently due to the state (psychical and physiological) of the observer. When he is especially active and free from fatigue despair never arises; but the opposite physical condition is almost sure to

give rise to feelings of both doubt and despair. None of these have been sufficiently regular or definite to guarantee our attaching any special significance to them.

2. A second stage, is characterized by imagery of various sorts, present in more or less profusion and in greater or less degree of intensity, or vividness. In a majority of the cases, vague schematic images of general or total situations, are first to appear. Their meaning is vague and becomes clear only with their growth in definiteness. One part of the image clears up, then another. Other images reinforce these, until the initial experience may be reinstated in almost complete form. The fact that these images are stable or recurrent, and the additional fact that they corroborate one another, though they may be incoherent and full of lacunae, arouse the 'feeling of familiarity' and signify to the observer that his procedure is one of recall and not one of creative imagination.

3. Our results show that the concrete objects or words of the initial perception, and the images employed during the act of solving problems are very much more likely to appear (in the form of images) in recall than are the 'short cuts' or processes of reasoning. The concrete images usher in meanings, and associations are made which then constitute the awareness of relation. The description of the imagery in the 'hog problem' (p. 72), and the Exs. 25 and 31 (pp. 67 and 74), illustrate this dawn of meaning and awareness of relations. These latter are clearly recalled in terms of coherent imagery. In other words, *content* was readily recalled while *act* was almost never recalled.

4. Very few of the unsuccessful trials in the solving are definitely recalled. The first unsuccessful trial that has been made is usually the only one ever recalled. This initial trial appears in about a third of the recalls; but the observer seldom fails to recognize that it had previously been present to consciousness as an error. The remembrance of other errors is present in an extremely vague and schematic fashion, *e.g.*, a visual image of splotches on the paper used in solving, etc.

5. The cues of an abstract and those of a concrete nature lead to approximately the same results.

6. Most observers tend to recall more data in visual imagery than was present in visual imagery during the solving. Some few observers show a slight tendency to recall in vocal-motor terms certain data which have been present during the initial experience in other modalities of sensory imagery. One observer employs in recall far more auditory imagery

than any other observer, and yet approximately only ten per cent. of her images during the act of solving are auditory.

7. The problems that deal with logical fallacies, and sentiments, are more frequently recalled in verbal imagery than are other problems. Here the verbal imagery constitutes about one-third of the content of recall; while in the recall of other problems, it constitutes less than one-tenth of the content.

8. The problems which present most novel features to the observer, as for example, 'the dot,' 'the spider,' and 'the density' problems, are recalled most predominantly in visual imagery.

9. The visual image is the strikingly dominant feature of recall. Feelings of familiarity and of satisfaction, pauses of attention and the like, are seldom recalled; they are never recalled except in connection with imagery. No recall of an affective state, of an attitude (*Bewusstseinslage*), of an *Aufgabe,* of steps in reasoning, or of a concept has been reported, except with images. From an actual count of factors present in the recall of ten of our problems, we estimate that our investigation embraces approximately 200,000 images but only 100 other components; and these latter are for the most part of an affective character. Of all our introspective data, about ninety per cent. are visual images.

THE EFFECTS OF STRYCHNINE ON MENTAL AND MOTOR EFFICIENCY

By A. T. Poffenberger, Jr., Ph. D., Columbia University

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I. Introduction

The present investigation was undertaken to determine the effect of ordinary doses of strychnine, such as are used for therapeutic purposes, on mental and motor efficiency, and to provide material for a comparative study of the effects of strychnine and caffeine on these processes. With the latter purpose in view, the tests and their conditions were made comparable, as far as possible, with those used by Dr. Hollingworth in his recent study.[1] Where changes were made they were to take advantage of the experience gained in the previous study, or were such as were made necessary by the smaller number of subjects.

I am indebted to Professor H. A. Hare of Jefferson Medical College, Philadelphia and to Dr. W. H. McCastline of Columbia University for advice concerning dosage and administration of strychnine; to Professors J. McK. Cattell and R. S. Woodworth for assistance in compiling results; to Dr. H. L. Hollingworth of Columbia University who gave me the benefit of his experience in testing the action of drugs and who arranged the schedule of doses. I am especially grateful to

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[1] H. L. Hollingworth, The Influence of Caffeine on Mental and Motor Efficiency. *Archives of Psychology,* No. 22, 1912.

Miss Gladys Tallman, A. M., of Columbia University who acted as subject and experimenter throughout the whole series of experiments.

a. Historical

The effects of strychnine as they are described in works on pharmacology and therapeutics[2] are as follows:

Small doses of strychnine improve the appetite and give a generally strong and hopeful feeling. With a full medicinal dose the respiration is quickened and deepened, the force of the pulse and the blood pressure are increased, but the temperature of the body is not changed except where poisonous doses are given. There is disagreement in regard to its effect on the pulse-rate. However Cushny says that the pulse-rate is reduced and the arterial tension increased. Strychnine exerts its greatest effect on the nervous system where its action is confined almost wholly to the centres in the cord and medulla, although the brain is also somewhat affected.[3] The field of its action is extended by most authors to include every region where simple reflexes may take place. The exact point of its action in the cord is not definitely settled. Some investigators say that it acts neither on the cells of the motor horn nor on the synapses around the motor cells, but that its action consists in the lowered resistance in other synapses, thereby extending the area of the action of the impulses. Others attribute to it an increase in the receptive activity of the sensory centers of the cord, and an increase in the conductive power of both motor and sensory nerves.[4] The action on the cerebrum is said to be very slight. Cases are cited in which fatal doses were taken and the intellect remained unclouded until death. There does not seem to be any direct action on the voluntary muscles, the increase of tone which results being due to an increase in the irritability of the centers in the cord.

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[2] Roberts Bartholow, *Materia Medica and Therapeutics,* New York, 1889, 431 ff.

G. F. Butler, *A Textbook of Materia Medica, Pharmacology and Therapeutics,* Phila. and London, 1910, 229 ff.

A. R. Cushny, *A Textbook of Pharmacology and Therapeutics, or The Action of Drugs in Health and Disease,* Phila. and New York, 1910, 198 ff.

H. A. Hare, *A Textbook of Practical Therapeutics,* Phila. and New York, 1912, 374 ff.

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R. W. Wilcox, *Materia Medica, Pharmacy, Pharmacology and Therapeutics,* Phila., 1904, 394 ff.

H. C. Wood, *Therapeutics: Its Principles and Practice,* Phila. and London, 1902, 207 ff.

[3] Robert W. Lovett, in "An Experimental Investigation of Strychnine Poisoning," (*Jour. of Physiol.,* IX, 1888, 99ff), performed experiments to determine why the effects of strychnine seemed to be localized in the cord, *i.e.,* to determine whether the cord is more susceptible to a given amount of strychnine than the other organs of the body, or whether it has the power of selecting strychnine from the circulation. An elaborate series of experiments seemed to show that when frogs have been given poisonous doses of strychnine and then killed and their organs examined, relatively more of the drug is found in the cord than in the brain or in any other organ of the body. He concluded that the cord has this power of attracting strychnine; but he did not reach any conclusion as to its relative susceptibility to the drug.

The physical symptoms with a full medicinal dose are slight shuddering, sense of constriction of the fauces and jaws, sudden pains like electric shocks passing through the limbs, startings of some of the voluntary muscles, dilated pupils, a meaningless smile, paleness of the face followed by flushing and increased warmth of the surface, and perspiration. Although hearing and smell are increased in acuteness, the greatest effect is noticed in the case of touch and vision. The field of vision is enlarged, especially for blue; and differences in colors can be recognised which ordinarily seem identical.

The effects differ according to the method of administering the drug. Hypodermic injections are the most active, and the most rapid in their effects. Dissolving tablets under the tongue is probably next in efficiency, and gives results in fifteen to twenty minutes. Taking the strychnine into the stomach gives the most tardy results, on account of the slower rate of absorption there. The absorption-time depends on the condition of the stomach, especially the presence of food. It requires from thirty to forty minutes to get results when the dose is taken into the stomach.

The dose usually prescribed varies from 0.01 to 0.05 grain. In emergency 0.1 grain may be given. One-half grain is the smallest dose known to be fatal; but from 0.5 to 1.5 grains is considered by all authors to be fatal. Only a very slight degree of tolerance is developed for strychnine, even after very prolonged administration.

Cushny,[5] in comparing the effects of caffeine, atropine and strychnine, says that each one of the drugs affects the whole nervous system but not equally. Strychnine affects the lower divisions, the spinal cord and medulla most strongly, showing itself in contractions or twitchings of isolated muscles; caffeine affects the highest functions, the psychical, with little motor effect; atropine holds a middle position and affects chiefly the motor division of the brain and shows itself in excessive coördinated movements, such as walking, talking, etc.

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[4] The experimental work of Houghton and Muirhead, "A Contribution to the Study of Strychnine Tetanus," *Medical News,* LXVI., 1895, 612ff, indicates that strychnine acts somewhere between the ganglia of the posterior root and the cells of the motor horn of the cord.

[5] A. R. Cushny, *Pharmacology and Therapeutics,* Phila. and N. Y., 1910, 284-285.

There have been very few researches on the effects of strychnine which have any direct relation to this experiment, and all of these consist of tests of fatigue by means of the ergograph. Rivers[6] mentions only three. Rossi,[7] working with the ergograph in tests lasting one hour, found a decided increase in the amount of work done, but there was no reaction at the end of the hour. Féré[8] also with the ergograph used subcutaneous injections of 8 milligrams of sulphate of strychnine, five minutes before the test began. He found a decided increase in work, followed by a fall below the normal. An initial decrease in the ergogram he explains as due to the effect of the injection process, and not to the drug. P. C. V. Jones[9] carried on a more extended set of experiments on the effects of strychnine on the ergograph curve. The experiments covered a period of fifteen days and were made up of six sets of six ergograms each. Some of the ergograms were made at half-hour intervals and others at hour intervals, so that the effect of the drug was followed in some cases for five and a half hours. The doses, taken by the mouth, were 4.2 milligrams of hydrochloride of strychnine (about one-fifteenth grain) during four days, and 1.8 milligrams, during four days, while on the remaining days the dose was made up of a mixture containing gentian, which could not be distinguished in taste from strychnine. With both doses there was considerable effect. The larger dose gave a rapid rise in the amount of work followed by a gradual fall; but the curve did not fall below the normal until the fifth and sixth, that is, the last two sets of ergograms. The curve for the smaller dose rose less rapidly and fell below the normal only during the last set. His general conclusion is that the effect of strychnine on the amount of work done with the ergograph is a decided increase followed by a decrease. He recognises the cumulative action of the drug, which shows itself in a general lowering of the curve in the second half of the experiment. One subject was used throughout the experiment. It is to be noted that Féré says the increase in the amount of work done is due to an increase in the *number* of contractions; also that Jones says that the increase is *mainly* due to this cause, and only slightly to an increase in the *height* of the contractions. He concludes thus:[10] "Strychnine acts essentially on the spinal cord and medulla; indeed it is remarkable that its action is so clearly defined on this tissue alone, for even in poisonous doses, it does not appear to affect the cerebrum or peripheral nervous structures. Moreover we now know that the drug acts peripherally on the sensory or receptor cells, through which it may be regarded as producing a change whereby impulses can pass through more easily and, instead of being localized to a relatively few efferent nerves, may break down the resistance so as to spread to all efferent fibers supplied from the spinal cord. I conclude then that the effects described are produced by the action of strychnine in diminishing the resistance to afferent impulses on the sensory side of the cord. The after effect may be due either to a poisonous effect of the drug or to pure fatigue."

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[6] W. H. R. Rivers, *Influence of Alcohol and Other Drugs on Fatigue.* London, 1908, 110-112.

[7] C. Rossi, Ricerche sperimentali sulla fatica dei muscoli umani sotto, l'azione dei veleni nervosi. *Riv. sper. di. freniat.,* XX, 1894, 442-480.

[8] C. S. Féré, *Travail et plaisir,* Paris, 1904, 297.

[9] P. C. Varrier-Jones, Effect of Strychnine on Muscular Work, *Jour. of Physiol.,* XXXVI, 1907-8, 435-446.

b. Conditions of the Experiment

The experiments to be reported in this paper covered a period of 38 days, not including a practise period of 10 days by subjects *A* and *B* before the regular work began. During this practice period each test was performed 100 times by *B* and 35 times by *A*. As a result *B* had practically reached the limit of practice, while *A* had at least passed beyond the point of rapid fall in the practice curve. The other subjects had no special practice. Although Rivers, in describing the conditions of fatigue experiments, considers such preliminary practice unnecessary, in these tests where fatigue is not the principal problem considered, preliminary practice removes one complicating factor.

The tests were conducted in a room in the Psychological Laboratory of Columbia University, free from disturbance. The subjects abstained from the use of tobacco, coffee, tea and cocoa during the experiment and for a period of two weeks before it began. They kept as regular hours of retiring and rising as their work permitted. Each kept a general health book in which daily records were made of the state of the health, the amount of sleep and any introspections which might seem of value. The subjects knew the purpose of the investigation,—that it was to study the effects of strychnine on their ability in the various tests. However, none knew the arrangement of the doses, so that any effect of the interest in taking a capsule which might have strychnine in it would be as likely to occur on a control day as on a strychnine day.

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[10] *Loc. cit.*, p. 446.

Seven subjects took part in the experiments; but in the main period covering thirty days, only two took part. A larger number was not available because the tests required such a large amount of the subjects' time. But by taking advantage of the series of experiments conducted by Dr. Hollingworth, on *The Influence of Caffeine on Mental and Motor Efficiency,* these two subjects were made to give a wide range of data. In that experiment there were 16 subjects, 15 of them being divided into four squads. All of the squads took their tests at the same time, but the doses of caffeine were given at different times of the day, and on different days, while one squad received no caffeine, that is, received control doses. In this experiment the schedule of the strychnine doses was so arranged that they were taken at the time of day found to be most favorable in the caffeine tests, namely, mid-afternoon. In addition to this, the doses were given in such series that the various combinations obtained with the larger number of subjects were duplicated with the two subjects at different stages of the experiment. All of the subjects were familiar with psychological work. In those tests in which only two subjects took part, one acted as experimenter while the other was subject. This meant checking up errors in some of the tests and taking the time for each of the tests. No difficulties arose from this method; and it had the special advantage of making a third person to act as experimenter unnecessary.

The strychnine was taken into the stomach in capsule form. Although there are objections to this method, in that the absorption time in the stomach depends on several factors, it seemed preferable to hypodermic injections or to dissolving the strychnine in the mouth. The first method is unpleasant and brings such a prompt reaction to the drug, that its effects may be complicated with the effects produced by administering the drug. Such disturbance was reported by Féré. The latter method is both unpleasant and offers the difficulty of providing a control tablet indistinguishable in taste from the strychnine. The control capsules contained milk sugar, and could not be distinguished in taste and appearance from the strychnine capsules. One capsule, either a control or a strychnine capsule, was taken each day. This ignorance of the nature of the dose reduces to a minimum the factors of suggestion, excitement and interest which might be present if the dose were known. The capsules were prepared by a reputable pharmacist and they were independently analyzed by the Columbia College of Pharmacy. They were found to contain the full quantity of pure strychnine sulphate with the slight variation common in such preparations, about 8 per cent. Doses of two sizes were given, one-thirtieth and one-twentieth of a grain. This was to determine the effects of different sized doses, and also to adjust the size of the dose to the body-weight of the subject.

The investigation may be divided into three sections: Experiment A, consisting of the first 28 days with two subjects; Experiment B, consisting of two days of intensive work of 12 hours each, with tests at half hour intervals; Experiment C, with 7 subjects and consisting of two half-days of intensive work. These experiments will be treated in the following sections:

II. Experiment A

This section of the experiment consisted of a series of tests covering a period of 28 days, from February 6, to March 5, inclusive. On each day during this period, subject *A* went through the tests 4 times, at the following hours: 9.30 A. M., 1.30, 3.30 and 5.30 P. M. Subject *B* had one additional test at 8.30 P. M. each day. A capsule was taken each day at 2.45 P. M. This arrangement gave two tests before the dose and two after it should have become effective (three in the case of Subject *B*). Each test lasted about one half-hour, and in the interval the subjects were free to attend to their regular work. The schedule of doses differed for the two subjects and will be given in Table I.

The first four days are recorded as control days for subject *A*.[11] Strychnine days and control days then alternate until Feb. 17, when two control days come together. After this strychnine and control days again alternate until Mar. 1. For subject *B*, the two kinds of doses alternate during the first three weeks, with the result that until Feb. 17, the two subjects receive strychnine on alternate days, while from this date to Mar. 1 they receive it on the same day. Strychnine was then taken by both subjects for three successive days, Mar.

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[11] The first two and the fourth day were intended to be control days for *A*, but the subject failed to take the capsule on the third day, which happened to be the first strychnine day.

1, 2, 3, followed by two control days, Mar. 4 and 5. This schedule was arranged by Dr. H. L. Hollingworth and was not disclosed to the subjects until the end of the experiment.

TABLE I

Schedule of Doses for Experiment A

Subject	*A*	*B*	*Dose*
Feb. 6..	C	C	1/30 gr.
7..	C	S	“
8..	C	C	“
9..	C	S	“
10..	S	C	“
11..	C	S	“
12..	S	C	“
13..	C	S	1/20 gr.
14..	S	C	“
15..	C	S	“
16..	S	C	“
17..	C	S	“
18..	C	C	“
19..	S	S	“
Feb. 20..	C	C	1/20 gr.
21..	S	S	“
22..	C	C	“
23..	S	S	“
24..	C	C	“
25..	S	S	“
26..	C	C	“
27..	S	S	“
28..	C	C	“
Mar. 1..	S	S	“
2..	S	S	“
3..	S	S	“
4..	C	C	“
5..	C	C	“

The first column in this table gives the date, the second and third columns describe the dose, and the fourth column shows the size of the dose. S stands for a strychnine day and C stands for a control day. For example, on Feb. 13, *A* had a control dose and *B* had a strychnine dose of 1/20 gr.

During this section of the experiment, a capsule was taken each day at 2.45 P. M., two hours and three-quarters after lunch, when under ordinary circumstances the stomach would be empty. The fourth column of Table I shows the size of the dose, 1/30 grain during the first week, and 1/20 gr. during the remainder of the experiment. Thus there were two tests each day before the dose was taken and two after it should have become effective. (In the case of *B* there were 3 after the dose.)

a. Description of the Tests

Eight tests were used: the steadiness test, tapping test, and three-hole test as a measure of motor efficiency; and the color-naming test, opposites test, addition test, cancellation test and multiplication test were used as a measure of mental efficiency. They are the same as the series given in the caffeine test except that the reaction experiment, the size-weight illusion test and the type-writing test are omitted as being the least satisfactory; and the multiplication test was added in order to give a test of higher mental ability than that afforded by the addition test.

The steadiness test offers the task of holding a metal rod 2.5 mm. in diameter in a hole 6 mm. in diameter for one minute. The subject stands with arms and body free from support during this test. Each contact of the rod with the sides of the hole is recorded on an electric counter. The test is intended to show the general stability of the nervous system. Since the body receives no lateral support, any tremor or twitching of the upper part of the body is likely to be recorded. The test is influenced by a great many uncontrollable factors; but it ought to be of value where the influence of a drug on the nervous system is to be recorded, since any such effect would be expected to show itself in a loss of balance of the delicate muscles. The results of the test are given in the number of contacts made during one minute.

The tapping test measures the time required for the subject to tap a metal plate with a metal rod 400 times. The number of contacts is measured by an electric counter, and the time taken in fifths of a second with a stop-watch. Since the speed of movement of a given set of muscles depends on the nervous discharge to those muscles, a drug which affects the resistance in the nerve centers ought to show its effects in the tapping test. And the most probable effect of such a drug is a change in resistance or conductivity of the nerve substance.

The three-hole test affords a combined measure of accuracy and speed. It consists in inserting the metal rod in each of three holes in a metal plate, until 100 insertions have been recorded on the electric counter. The plate is tilted toward the subject at an angle of 45 degrees; and the holes are located at the corners of an equilateral triangle, about 8 cm. apart. During the test, the subject was seated before the apparatus, with the body unsupported by the table. This test requires considerably more muscular control and coördination than either of the former. The results show the number of seconds required to make the 100 insertions.

The mental tests[12] consisted of a series of association tests, all of them common in psychological work. The simplest one is the color-naming test which measures the speed with which the subject can recognise and name the five most common colors, red, yellow, green, blue and black. These are arranged on a card in the form of a square, in which each color appears 20 times. The time required to name the 100 colors was taken with the stop-watch; and this is the unit in which the results are reported. Four color-orders were obtained by turning the card around 90 degrees at each reading. On account of the long preliminary practice, the subjects were perfectly familiar with the colors and their names, so that the test measured the speed with which the name could be spoken when the color was seen, that is, the speed of the association process. The variation of the colors under differing light conditions made it necessary to conduct this test under electric light. As in all of the association tests, the responses were spoken; and the subject was made to correct his errors before continuing the test.

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[12] An experimental study of these tests is reported in Woodworth and Wells, Association Tests, *Psychol. Rev., Mon. Sup.*, 1911, VIII, 5. A discussion of the same tests and the motor tests, with reference to their use for purposes like the present one, will be found in Hollingworth's, Influence of Caffeine on Mental and Motor Efficiency, *Archives of Psychology*, No. 22, 1912.

The method of conducting the tests is that generally used; it follows quite closely the method described by Dr. Hollingworth, so that the procedure will not be given in detail here.

The opposites test consisted in naming, as rapidly as possible, the opposites of 50 adjectives, taken from the 'moderately difficult' list of Professor Woodworth. These words were arranged in 10 chance orders; and the same list was repeated every two days. The test measured the speed of the association process between two ideas, the subjects having become familiar with the words and their opposites during the preliminary practise period. The tables show the number of seconds required to name the 50 opposites.

The calculation tests, addition and multiplication, afford a still more complicated association process, in that the mental operation is not so definitely predetermined. However, after these tests have been repeated a great number of times, they approach more and more the type of the opposites test, that is, the sight of the number calls up the answer directly. The addition test consisted in the addition of 17 to each of 50 two-place numbers, ranging from 20 to 80, with the numbers ending in 0 omitted. Ten chance orders were prepared as in the opposites test, and the results are given in seconds.

The multiplication test consisted in multiplying each of 25 two-place numbers by 7. The same lists of numbers were used as in the addition test, except that only half of a list was given at one time. This provided 20 arrangements of the 25 numbers, and the same list was repeated every four days.

The cancellation test was used as a measure of the higher mental processes, such as attention, discrimination, etc. The cancellation blank consisted of 1,000 numbers, each of the

TABLE II

EXPERIMENT A

Averages and Differences for Each Test Period

(The figures in the table represent the average, with its probable error, P.E., of all the control days, C, for a given test period, and the average of all the strychnine days, Str., for a given test period. The minus sign in the columns of differences, Diff., indicate that the strychnine test was the faster, except in the steadiness test, where it means a smaller number of contacts. The average differences appear in heavy type.)

Test	*Subject*		9.30			1.30			*Av. Diff. Before Dose*
			C.	*Str.*	*Diff.*	*C.*	*Str.*	*Diff.*	
OPPOSITES.....	A	Av.	38.4	37.3	—1.1	36.3	36.6	+0.3	**—0.4**
		P.E.	1.3	1.0	1.6	0.8	0.9	1.2	
	B	Av.	29.0	28.6	—0.4	28.8	27.8	—1.0	**—0.7**
		P.E.	0.4	0.3	0.5	0.6	0.3	0.7	
COLOR-NAMING	A	Av.	43.4	42.1	—1.3	44.9	41.8	—3.1	**—2.2**
		P.E.	0.6	0.5	0.8	0.9	0.6	1.1	
	B	Av.	36.0	34.7	—1.3	37.4	35.5	—1.9	**—1.6**
		P.E.	0.6	0.4	0.7	0.6	0.5	0.8	
CANCELLATION.	A	Av.	51.8	51.7	—0.1	52.3	52.9	+0.6	**+0.3**
		P.E.	1.3	1.2	1.8	0.5	1.0	1.1	
	B	Av.	46.6	48.1	+1.5	38.9	38.1	—0.8	**+0.4**
		P.E.	1.3	1.5	2.0	0.5	0.6	0.8	
ADDITION......	A	Av.	67.0	65.9	—1.1	65.8	64.3	—1.5	**—1.3**
		P.E.	1.7	2.3	2.9	1.7	2.0	2.6	
	B	Av.	43.0	40.0	—3.0	41.3	40.3	—1.0	**—2.0**
		P.E.	1.2	0.7	1.7	1.3	1.3	1.8	
MULT.........	A	Av.	63.1	60.3	—2.8	65.4	62.4	—3.0	**—2.9**
		P.E.	2.0	2.2	3.0	1.6	1.9	2.5	
	B	Av.	42.8	41.7	—1.1	43.9	42.8	—1.1	**—1.1**
		P.E.	1.0	1.0	1.4	0.7	0.8	1.1	
TAPPING	A	Av.	59.0	56.4	—2.6	57.4	58.3	+0.9	**—0.9**
		P.E.	1.4	0.9	1.7	0.8	0.9	1.2	
	B	Av.	38.9	38.8	—0.1	38.3	38.6	+0.3	**+0.1**
		P.E.	0.2	0.1	0.2	0.2	0.3	0.4	
THREE-HOLE..	A	Av.	42.7	42.9	+0.2	42.5	43.0	+0.5	**+0.4**
		P.E.	0.4	0.4	0.6	0.4	0.3	0.5	
	B	Av.	40.5	40.7	+0.2	39.7	40.6	+0.9	**+0.6**
		P.E.	0.4	0.4	0.6	0.3	0.5	0.6	
STEADINESS ...	A	Av.	16.2	15.6	—0.6	11.3	12.8	+1.5	**+0.5**
		P.E.	1.6	0.8	1.8	0.9	1.3	1.6	
	B	Av.	17.2	13.1	—4.1	12.2	12.9	+0.7	**—1.7**
		P.E.	1.7	1.1	2.0	1.3	0.9	1.6	

TABLE II—(Continued)

EXPERIMENT A

Averages and Differences for Each Test Period

3.30			5.30			*Av. Diff. After Dose*	*Diff. Due to Str.*
C.	*Str.*	*Diff.*	*C.*	*Str.*	*Diff.*		
36.8 0.8	36.9 1.0	+0.1 1.3	37.9 0.8	36.1 0.8	—1.8 1.1	**—0.9**	**—0.5**
28.6 0.5	29.3 0.7	+0.7 0.9	29.1 0.5	28.7 0.6	—0.4 0.8	**+0.2**	**+0.9**
42.7 0.8	41.5 1.0	—1.2 1.3	42.6 0.6	43.6 0.8	+1.0 1.0	**—0.1**	**+2.1**
37.1 0.7	36.4 0.5	—0.7 0.9	37.7 0.7	36.8 0.4	—0.9 0.8	**—0.8**	**+0.8**
51.1 0.8	53.6 1.1	+2.5 1.4	54.6 1.8	54.5 2.2	—0.1 2.8	**+1.2**	**+0.9**
41.1 0.7	42.0 0.6	+0.9 0.9	51.5 2.4	49.3 2.0	—2.2 3.1	**—0.7**	**—1.1**
66.6 1.8	65.6 2.1	—1.0 2.8	66.6 1.6	65.3 1.6	—1.3 2.3	**—1.2**	**+0.1**
40.5 1.2	39.9 1.6	—0.6 2.0	42.5 1.5	41.6 1.6	—0.9 2.2	**—0.8**	**+1.4**
68.2 1.8	63.1 2.0	—5.1 2.7	64.0 1.6	63.2 1.2	-1.7 2.0	**—3.4**	**—0.5**
43.7 0.9	40.8 1.1	—2.9 1.4	44.0 1.1	46.3 1.5	+2.3 1.9	**—0.3**	**+0.8**
58.6 1.1	59.6 1.1	+1.0 1.6	58.6 0.4	57.9 0.9	—0.7 1.0	**+0.2**	**+1.1**
38.3 0.2	38.3 0.2	0.0	38.9 0.3	38.1 0.2	—0.8 0.4	**—0.4**	**—0.5**
42.6 0.3	42.3 0.2	—0.3 0.4	43.4 0.3	42.3 0.4	—1.1 0.5	**—0.7**	**—1.1**
40.6 0.7	40.0 0.7	—0.6 1.0	41.8 0.6	43.4 0.5	+1.6 0.8	**+0.5**	**—0.1**
9.3 0.6	10.7 0.5	+1.4 0.8	12.8 1.0	13.9 0.9	+1.1 1.3	**+1.2**	**+0.7**
10.1 1.0	9.8 0.7	—0.3 1.2	16.1 1.5	17.7 0.7	+1.6 1.7	**+0.7**	**+2.4**

digits from 0 to 9 repeated 100 times. One-half of the blank was used in each test, and the results show the time required to cancel a given digit from this list. The difficulty of accounting for errors was avoided by informing the subject that he must cancel five digits in each line. On account of the difference in the legibility of the different digits, only 2, 3 and 5 were used in the latter part of the work. These three have been found to be of about equal difficulty.

b. Discussion of the Data

The data of the entire Experiment A (with the exception of the 8.30 test period) are given in Table II. The first column in the table shows the name of the test, and the second column shows the subject. The rest of the table may be divided into two main parts, the first part containing the data for the 9.30 and 1.30 test periods, that is, the two before the dose was taken; and the second part containing the 3.30 and 5.30 test periods, or those after the dose was taken.[13] Since the 8.30 test was given to only one subject, the data will be given in a separate table (III). The data for this test period show the same results as those in Table II. Under each of the four test periods, there are three columns marked *C, Str.* and *Diff.* In the columns marked *C* are the average times for every test at the given test period on *control days* throughout Experiment A; in the columns marked *Str.* are the average times for every test at the given test period on *strychnine days;* and under *Diff.* will be found the difference in time between this control average and the strychnine average. The number of individual records making up these averages may be seen by referring to Table I, but it is approximately 15. In every case the control average is subtracted from the strychnine average, so that a *minus sign* means that a strychnine day was *faster* than the control day, that is, the average time required to perform the test was shorter; while the plus sign means that the average time was longer on the strychnine day. All the figures represent time except in the steadiness test, where they represent the number of contacts in a given time; and the minus sign means fewer contacts, or a better record on strychnine days. Below each average and each difference, its probable error (P.E.) is given. For example, in the opposites test, at the 9.30 period, subject *A* had an average time on control days of 38.4 seconds, with a P.E. of 1.3, and on strychnine days, an average of 37.3 seconds, with a P.E. of 1.0, and the strychnine average for this period is 1.1 seconds faster than the control average, with a P.E. of 1.6.

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[13] The results from the 1/30 grain were so similar to those from the 1/20 grain that they have all been combined into the one average shown in the table; and to avoid complication the dose is marked 1/20 throughout. Reference to Table I will show when the 1/30 grain was taken.

Following the 1.30 test period there is a column marked *Av. Diff. Before Dose,* which shows the average of the two differences before the dose. Following the 5.30 test period, there is a column containing the average differences after the dose, obtained in the same way.

Since the dose in every case was taken at 2.45 P. M., the first two tests of the day, 9.30 A. M. and 1.30 P. M., would be unaffected by it, so that even on strychnine days the first two tests were control tests. Therefore, there are not only full control days, but the first half of each strychnine day forms a control for that particular day. The value of this check is evident from the table which shows considerable variation in the morning tests from day to day. If conditions were perfect, the difference between control days and strychnine days in the 9.30 and the 1.30 tests should be zero, or at least the difference should be equally divided between plus and minus differences, since the influence to be measured is not then effective.[14] However, as this difference does appear, it must be considered in calculating the difference between control and strychnine days after the dose. Any difference which is to be attributed to the strychnine must be the amount beyond the difference occurring before the dose became effective. That is, a difference of two seconds in favor of the strychnine day at the 3.30 test period would be of no significance if there were also such a difference before the dose was taken. The last column in the table marked *Diff. Due to Str.,* takes this factor into account, and shows the average difference between a control and a strychnine day after the dose, beyond the difference occurring before the dose. The figures in this column are obtained by subtracting the average difference before the dose from that after the dose. As before, the minus sign means that the average of the strychnine days after the dose was faster than that of the control days. The whole process

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[14] A study of the data showed that the morning variations were not consistent enough to be the result of a dose of strychnine on the previous day. No reason is given for the fact that the first two tests of strychnine days are so often faster than the same tests on control days, as seen by the predominance of minus signs in these differences. But as most of the differences are within their P.E. the sign is of no importance.

Fig. 1

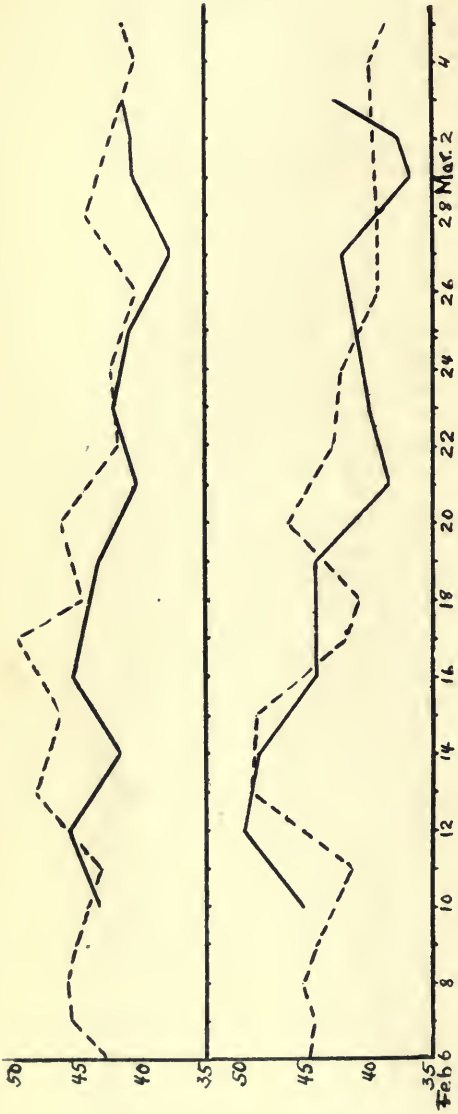

Fig. 2

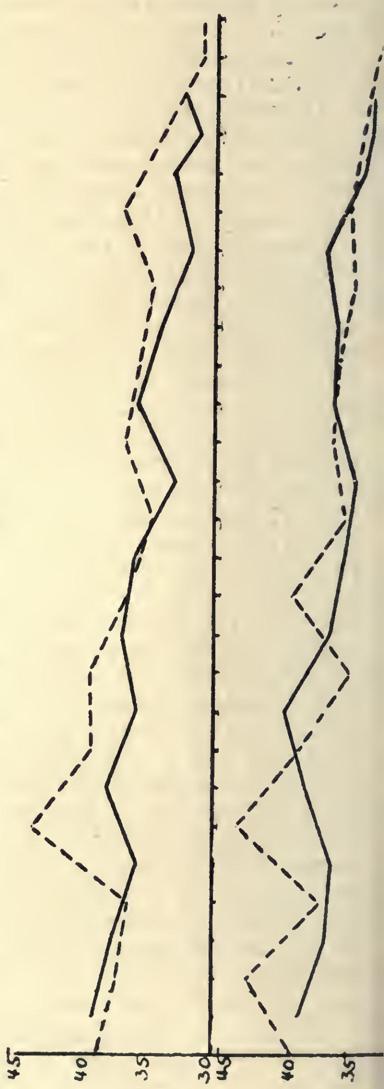

Fig. 1—Curves showing daily records in the Color-Naming Test for Subject *A*.

Fig. 2—Curves showing daily records in the Color-Naming Test for Subject *B*.

[Broken lines represent control days and solid lines represent strychnine days. The upper pair of curves represents tests before the dose and the lower pair those after the dose on control (broken) and strychnine (solid) days.]

by which this last column is derived may be expressed in the form of an equation, which may make the method clearer:

$$\frac{3.30\ (Str.-C)+5.30\ (Str.-C)}{2}-\frac{9.30\ (Str.-C)+1.30\ (Str.-C)}{2}=\mathit{Diff.\ due\ to\ Str.}$$

The differences thus found are extremely small,—in 10 cases out of 16, one second or less. In three of the six cases where the difference is above 1 second (Cancellation, Tapping and Three-Hole) the signs are different for the two subjects. That is, the average strychnine time is shorter for the one subject and longer for the other. In the other three cases (Color-Naming, Addition and Steadiness), although the signs are alike for both subjects, the values for one subject in each case are below the probable error.[15] That is, in no two cases are the differences for the two subjects above the P.E. and of like sign. In the Color-Naming test for Subject *A*, the difference due to strychnine is 2.1 seconds. This difference is beyond the probable error and is the most reliable of all those given in the table.

Figs. 1 and 2 show graphically the daily records of subjects *A* and *B* respectively in the Color-Naming test. The curves of this test are selected as typical of all the others. The ordinates represent time in seconds, and the abscissas represent the date of the test. *Broken lines* signify *control* days and *solid lines* signify *strychnine* days. In each case, the upper set of curves shows the average of the two tests before the dose on strychnine days (solid), and on control days (broken). The lower set of curves shows the average of the two tests after the dose on control days (broken), and on strychnine days (solid). Relative difference in the position of the solid line with reference to the broken line in the curves before and after the dose would be due to the drug. Neither pair of curves is significant taken alone, but only when the relative position of the solid line in the two cases is compared. For example, if the solid line in the lower set of curves lay entirely below the broken line and in the upper set intersected a number of times, it would indicate that the strychnine days were uniformly faster than control days. However, the crisscrossing of the two lines before the dose and after the dose shows that there is no influence working only in the afternoon.

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[15] There is no well-known method available for finding the reliability of these final differences, as they are the differences between the sums of averages; and we cannot calculate the P.E. of the sums of two averages. However, inspection of the reliability of the averages makes it clear that the differences are, in most cases, not far enough above what we might expect by chance to interpret these final differences as the effect of strychnine.

Table III gives the data for the 8.30 test period in which *B* was the only subject. The probable errors are higher than for the other test periods, so that the final differences are less reliable. These data serve to corroborate the results shown in Table II.

The conclusion to be drawn from Experiment A is that, for the two subjects tested, ordinary doses of strychnine, taken by the mouth, produce no effect measurable by the tests employed.

TABLE III

Averages and Differences for 8.30 *Test Period*

(Only subject *B* took part in the 8.30 tests. The figures and signs have the same meaning as indicated for table II.)

Test	*C.*	*Str.*	*Diff.*	*Test*	*C.*	*Str.*	*Diff.*
OPPOSITES...	29.2	28.5	—0.7	MULT.......	44.8	41.6	—3.2
	0.4	0.4	0.6		1.4	0.9	1.7
COLOR-NAM..	36.3	35.6	—0.7	TAPPING.....	38.9	38.7	—0.2
	0.5	0.4	0.6		0.2	0.2	0.3
CANCELL....	41.5	41.6	+0.1	THREE-HOLE	43.2	44.6	+1.4
	1.3	0.8	1.5		0.5	0.3	0.6
ADDITION ...	43.1	42.9	—0.2	STEADINESS .	20.8	22.4	+1.6
	1.2	1.1	1.6		0.9	1.5	1.7

III. Experiment B

a. Procedure

The second part of the investigation, to be known as Experiment B, was planned to give a more complete record of the action of strychnine, to discover any very transient stimulation which might have fallen between test periods in Experiment A, and to detect any decrease in efficiency below the normal which might follow as a secondary reaction upon an increase in efficiency. It consisted of a series of tests covering two days in which the two subjects *A* and *B* took part. The same tests and the same technique were employed as in Experiment A. There were twenty-three tests each day, beginning at 8.30 A. M. and following at half hour intervals until 8.30 P. M., with the exception of the 11.30 and 6.30 tests, which were omitted on account of lunch and dinner. As each test period lasted nearly a half hour, there was practically a continuous series of tests during the twelve hours. Each subject had one control day and one strychnine day. A capsule, either a control or strychnine capsule, was taken each day at 1.45 P. M. This arrangement gave 11 tests before the dose would become effective, and twelve during which the influence of the drug could be observed. A curve of efficiency for each day could thus be established before the capsule was taken, and the effects of the drug could be followed for six and one-half hours.

b. Discussion of the Results

The results of this experiment are given in Table IV. The general plan of the table is identical with that of Table II. The first column contains the name of the test, and the second designates the subject. The remainder of the table is divided into two parts, the first part including the 11 tests before the dose and the second part including the 12 tests after the dose. As the tests were given at half hour intervals, the number of the test and not its time is given at the top of the columns.

The derivation of the figures in this table differs somewhat from that of Table II. Since there were only two whole days included in the experiment, there could be only two tests for each test period; and one of these would be on a strychnine day and the other on a control day. The figures in the table, then, show the difference between these two tests for a given test period, the control day being always subtracted from the strychnine day. Hence a minus sign indicates that the time on the strychnine day was shorter than on the control day. Just as in Experiment A, there will be a certain number of test periods even on a strychnine day, which will be unaffected by the dose, namely, the first 11, since the tests were made before the dose was taken. These 11 tests then serve as a measure of the normal performance of the day. If only chance factors entered, the difference between control and strychnine days in the first 11 tests should be zero or the plus and minus values should be equally divided. At any rate the differences after the dose must be interpreted with reference to these differences before the dose. Here it may be objected that these first 11 tests do not represent a normal performance for the day, since considerable variation may occur later in the day under ordinary conditions. Any constant variation with the time of day will be checked by the complete control day. The establishment of a norm for each day is intended to serve

TABLE IV

EXPERIMENT B. INTENSIVE

Differences Between Strychnine and Control Days

(The figures in this table show the difference between the time of a given test on a control day and on a strychnine day, except in the case of the steadiness test, where the figures represent the difference in contacts. The minus sign indicates that the time on the strychnine day was the faster.)

TEST NO	*Subject*	*Tests Taken Before The Dose*											*Av. Diff. Before Dose*	*P.E.*
		1	2	3	4	5	6	7	8	9	10	11		
OPPOSITES	A	—1.6	—6.2	+ 0.2	+1.8	— 2.6	— 1.2	— 3.4	—4.0	—0.8	+4.0	—0.6	**—1.3**	0.6
	B	+4.2	+7.6	—1.6	+0.8	+0.2	+3.2	+2.2	—1.6	+1.8	+3.4	—0.2	**+1.8**	0.7
COLOR-NAM. .	A	—3.8	—0.4	+1.2	—2.6	+3.0	+2.0	—7.0	+0.2	—1.2	—3.2	—4.0	**—1.4**	0.6
	B	+4.8	+5.4	+2.4	+1.0	—0.8	—0.2	+0.4	+0.6	+2.4	+1.8	+4.0	**+2.0**	0.4
CANCELLATION.	A	—2.4	—4.4	—21.2	—4.4	—2.2	—24.0	+0.8	- - -	—0.8	—5.0	—3.2	**—6.7**	1.8
	B	—0.4	+5.2	—2.0	—5.2	—2.2	—3.0	+2.2	+1.2	—0.6	+3.3	—2.2	**—0.3**	0.6
ADDITION	A	+0.4	0.0	—0.2	—6.2	+12.2	+7.0	+5.2	—1.8	—4.2	+9.4	+0.2	**+2.0**	1.2
	B	+2.0	—0.8	—1.6	0.0	+3.6	0.0	+2.2	+1.0	—1.0	—0.2	—4.6	**+0.1**	0.4
MULT . . .	A	—1.8	—6.4	+20.0	—4.0	—2.8	+3.6	—11.6	+6.4	—5.6	0.0	+2.4	**0.0**	1.5
	B	—10.2	+0.4	—10.6	+1.6	+3.8	—4.2	—2.4	+7.4	—3.0	+5.8	—6.0	**—2.3**	1.1
TAPPING.	A	+8.6	+3.0	+4.6	—6.4	—2.6	—0.8	—2.4	—0.4	+1.2	—3.8	—6.4	**—0.5**	0.9
	B	—1.2	+0.8	0.0	+1.6	+1.6	+2.2	—0.8	—0.2	+0.8	+0.4	+1.4	**+0.6**	0.2
THREE-HOLE.	A	+0.8	+5.6	+1.8	—2.6	—1.4	—1.2	—0.2	—0.4	—1.6	—2.8	+1.2	**—0.1**	0.5
	B	+1.0	+1.0	+3.4	+3.4	+5.8	+3.4	+2.4	—2.2	+2.8	+2.6	+2.6	**+2.4**	0.3
STEADINESS	A	—9.0	—6.0	0.0	—5.0	—4.0	—12.0	+2.0	—2.0	0.0	—2.0	—6.0	**—4.0**	0.8
	B	+8.0	—2.0	+5.0	—6.0	—6.0	—4.0	—3.0	+4.0	—1.0	+2.0	—3.0	**—0.5**	0.1

TABLE IV—(Continued)

Test No. . .	Subject	*Tests Taken After The Dose*												*Av. Diff. After Dose*	*P.E.*	*Diff. due to Str.*	*P.E.*
		12	13	14	15	16	17	18	19	20	21	22	23				
OPPOSITES . .	A	—3.2	+3.6	—0.8	—2.4	—1.8	—3.0	+3.2	—0.6	—3.6	+2.0	—7.4	—7.0	**—1.8**	0.7	**—0.5**	0.9
	B	+8.4	—1.2	+1.8	+2.8	+3.8	+0.6	+3.2	+1.4	—0.2	+3.0	+2.6	+2.8	**+2.4**	0.4	**+0.6**	0.6
COLOR-NAM	A	—5.0	+7.6	+8.4	—4.6	—2.4	+0.2	—2.2	+1.4	+0.4	—3.0	+2.6	—2.6	**+0.1**	0.8	**+1.5**	1.0
	B	0.0	+5.0	+4.4	+5.8	+2.0	+1.4	+7.2	+6.8	+4.4	+1.4	—7.4	+2.2	**+2.8**	0.7	**+0.8**	0.8
CANCELL'N. .	A	—10.0	—4.2	—1.8	+11.8	+8.8	+10.6	+10.6	+1.2	—0.6	+19.0	—5.2	—7.2	**+2.8**	1.9	**—9.5**	2.4
	B	+7.6	+14.0	—0.4	—1.8	+1.4	—3.0	+7.2	+1.2	+0.8	0.0	—5.4	+3.4	**+2.1**	1.2	**+2.4**	1.3
ADDITION. . .	A	+0.8	+2.8	—5.8	—0.8	+3.6	+1.2	—4.2	—2.6	—0.4	—0.6	—10.6	+0.4	**—1.4**	0.7	**—3.4**	1.4
	B	+1.0	+4.4	+0.4	—3.8	—0.8	0.0	—0.4	+3.4	—2.4	—5.6	+5.2	+4.6	**+0.5**	0.7	**—0.5**	0.8
MULT.	A	—7.6	—23.6	+2.4	+5.0	+5.8	+7.8	—4.4	+14.4	+2.0	+8.8	—5.0	+11.4	**+1.4**	1.9	**+1.4**	2.4
	B	—2.4	—3.6	+2.8	—3.8	—4.0	—2.2	+8.8	—8.4	+4.8	—3.0	—5.2	—0.6	**—1.4**	0.9	**+0.9**	1.4
TAPPING . .	A	—4.2	—1.2	0.0	—3.6	—0.2	+2.8	—0.2	+0.6	—1.6	+2.6	+1.0	—4.2	**—0.7**	0.5	**—0.2**	1.0
	B	+0.6	+0.6	0.0	+1.0	—0.4	+0.2	+0.6	—0.2	+0.4	+0.8	+0.4	+1.6	**+0.5**	0.1	**—0.1**	0.2
THREE-HOLE	A	—0.2	+6.0	—1.8	—3.8	—5.0	+2.4	+1.2	—2.6	—2.8	—0.6	—0.4	—4.6	**—1.0**	0.6	**—0.9**	0.8
	B	—1.6	+0.4	+4.8	+3.8	+4.4	+0.2	+4.4	+5.6	+1.2	+3.0	—2.0	+6.4	**+2.6**	0.6	**+0.2**	0.7
STEADINESS.	A	—7.0	—12.0	0.0	+4.0	+6.0	—3.0	—4.0	—7.0	—4.0	—4.0	—13.0	+2.0	**—3.5**	1.1	**+0.5**	1.4
	B	+7.0	+4.0	+5.0	—2.0	0.0	+5.0	+2.0	—5.0	+9.0	+11.0	+6.0	+3.0	**+3.8**	0.9	**+4.3**	1.4

as a check on extreme changes in health or general conditions, such as might conceivably occur from one day to another, and I think that it serves this purpose.

Following the records of the first 11 test periods, the average of these differences is given in the column marked *Av. Diff. Before Dose,* and in the next column is the P.E. of this average difference. After test period No. 23, a similar column shows the average difference after the dose, with the P.E. of the average. The next to the last column in the table gives the results due to strychnine and represents the difference between the average difference before the dose and that after the dose. In this case the P.E. of the final value can be calculated and is given in the last column of the table. These figures correspond in meaning with the last column of Table II.

Let us consider the data for the various tests in this table. In the Opposites test the differences for the two subjects are in opposite directions and neither is above its P.E. In the Color-Naming test the difference for *A* is one and one-half times the P.E., while for *B* the difference does not exceed the P.E. In the Cancellation test the differences are quite large and beyond their P.E., but for one subject the strychnine day is faster and for the other it is slower. It is difficult to see how in a test like the Cancellation test strychnine should produce such opposite results in two individuals. I am inclined to attribute these large differences to some irregularity in method or material, especially since the differences were very small in Experiment A, and in the opposite direction in the case of each subject. There *A*'s strychnine time was slower and *B*'s faster. In the addition test *A*'s time is faster for the strychnine day by more than twice the P.E., while *B*'s time is less than the P.E. In Experiment A, the difference for *A* is much less and in the opposite direction, that is, while in Experiment B it is faster, in Experiment A it is slower. In no other case are the differences beyond their P.E., except in the Steadiness test for Subject *B,* where the strychnine record is considerably worse. However, in both experiments for both subjects the strychnine record is poorer, that is, there were more contacts, than in the control record. This is indicated by the positive signs, although the difference is small in all but the one case mentioned. These differences in the Steadiness test are the nearest approach to a difference which may be attributed to strychnine.

An objection might be made to the method of treating the results of this experiment. If strychnine does cause an excitation followed by a depression, by simply grouping the tests

TABLE V

Group Differences Between Strychnine and Control Days

(The column marked *Before Dose* is the average of all the differences between strychnine and control days in all the 11 tests made before the dose was taken. They are the same as the figures in table IV marked *Av. Diff. Before Dose.* The 12 tests after the dose were divided into 3 groups of 4 each and the average difference between strychnine and control days in these groups is shown in the last three columns of the table.)

	Subject		*Before Dose*	*After Dose*		
				Group I	*Group* II	*Group* III
Opposites	A	Av. Diff.	−1.3	−0.7	−0.6	−4.0
		P.E.	0.6	0.9	0.8	1.4
	B	Av. Diff.	+1.8	+3.0	+2.3	+2.1
		P.E.	0.7	1.2	0.5	0.5
Color-Nam	A	Av. Diff.	−1.4	+1.6	−0.8	−0.7
		P.E.	0.6	2.7	0.7	0.9
	B	Av. Diff.	+2.0	+3.8	+4.4	+0.2
		P.E.	0.4	0.8	1.1	1.6
Cancellation	A	Av. Diff.	−6.7	−1.1	+7.8	+1.5
		P.E.	1.8	2.7	1.4	3.7
	B	Av. Diff.	−0.3	+4.9	+1.7	−0.3
		P.E.	0.6	2.0	1.2	1.1
Addition	A	Av. Diff.	+2.0	−0.8	−0.5	−2.8
		P.E.	1.2	1.1	1.2	1.6
	B	Av. Diff.	+0.1	+0.5	+0.6	+0.5
		P.E.	0.4	0.9	0.6	1.9
Mult	A	Av. Diff.	0.0	−6.0	+5.9	+4.3
		P.E.	1.5	4.1	2.2	2.4
	B	Av. Diff.	−2.3	−1.8	−1.5	−1.0
		P.E.	1.1	1.0	2.1	1.3
Tapping	A	Av. Diff.	−0.5	−2.3	+0.8	−0.6
		P.E.	0.9	0.7	0.5	1.0
	B	Av. Diff.	+0.6	+0.6	+0.1	+0.8
		P.E.	0.2	0.1	0.1	0.2
Three-Hole	A	Av. Diff.	−0.1	+0.1	−1.0	−2.1
		P.E.	0.5	1.2	1.2	0.7
	B	Av. Diff.	+2.4	+1.9	+3.7	+2.2
		P.E.	0.3	1.1	0.7	1.1
Steadiness	A	Av. Diff.	−4.0	−3.8	−2.0	−4.8
		P.E.	0.8	2.5	1.7	1.8
	B	Av. Diff.	−0.5	+3.5	+0.5	+7.3
		P.E.	0.1	1.2	1.3	1.2

before the dose and those after the dose, the excitation and depression effects might be combined in the second group and neutralize each other. In order to show more clearly whether there is such a stimulation followed by a depression, the twelve test periods after the dose have been taken and divided into three groups of four test periods each. Group I, including the first four tests after the dose, represents from one-half to 2 hours after the dose; Group II, including the second 4 test periods, represents from 2 to 4 hours after the dose; and Group III, including the last 4 test periods, represents from 4 to 6 hours after the dose. By comparing each of these groups with the average before the dose, any progressive change in efficiency should appear. The data thus treated are given in Table V. The values with their P.E. under the first column are taken directly from Table IV. The values given under the different groups are derived as stated above.

It is necessary, in determining the true strychnine effect from this table, to compare the values in each of the groups with that before the dose in each case, just as was explained in connection with Table IV. As it is rather difficult to do this by inspection and at the same time judge of the reliability of the differences, Table VI has been compiled to show these differences in each of the groups, after subtracting from each of them the value before the dose. In Table VI, then, only the three groups after the dose are represented, each figure being the difference between *Before Dose* and a given group after the dose. The reliability of each of these differences is indicated by the P.E. Table V might have been omitted except that it would be very difficult to understand the derivation of Table VI without first having seen Table V.

Examination of this table shows that in the Opposites test no differences beyond the P.E. appear. In the Color-Naming test Subject *A* has no significant differences in the different groups, while *B* in the first two has differences about twice the P.E. This is just the reverse of Table IV, for there it was *A* who had the larger differences. In the Cancellation test the differences are still large but for subject *B* in Groups II and III are not much greater than the P.E. In the Addition test we find the same condition as stated in connection with Table IV, that is, the difference is the reverse of that found in Experiment A. No other large differences are found except in the Steadiness test for *B*, and the same was noted in Table IV. The most important thing to be observed in this table is the relation among the three groups. Comparing Group II with Group I we find that in six cases the first

TABLE VI

Values for the Three Groups After the Dose After Subtracting from Them the Average Difference Before the Dose

(This table is obtained directly from table V by subtracting the average difference before the dose from each of the three groups after the dose. It shows the effect of strychnine on each of the tests, the minus sign indicating a better record after taking strychnine.)

Test	*Subject*		*Group I*	*Group II*	*Group III*
Opposites	A	Diff.	+0.6	+0.7	—2.7
		P.E.	1.1	1.0	1.5
	B	Diff.	+1.2	+0.5	+0.3
		P.E.	1.4	0.9	0.9
Color-Nam	A	Diff.	+3.0	+0.6	+0.7
		P.E.	2.8	0.9	1.1
	B	Diff.	+1.8	+2.4	—1.8
		P.E.	0.9	1.2	1.6
Cancellation	A	Diff.	+5.6	+14.5	+8.2
		P.E.	3.2	2.3	4.1
	B	Diff.	+5.2	+2.0	0.0
		P.E.	2.1	1.3	1.3
Addition	A	Diff.	—2.8	—2.5	—4.8
		P.E.	1.6	1.7	2.0
	B	Diff.	+0.4	+0.5	+0.4
		P.E.	1.0	0.7	1.9
Mult	A	Diff.	—6.0	+5.0	+4.0
		P.E.	4.3	2.7	2.8
	B	Diff.	+0.5	+0.8	+1.3
		P.E.	1.5	2.4	1.7
Tapping	A	Diff.	—1.8	+1.3	—0.1
		P.E.	1.1	1.0	1.3
	B	Diff.	0.0	—0.5	+0.2
		P.E.	0.2	0.2	0.3
Three-Hole	A	Diff.	+0.2	—0.9	—2.0
		P.E.	1.3	1.3	0.9
	B	Diff.	—0.5	+1.3	—0.2
		P.E.	1.1	0.8	1.1
Steadiness	A	Diff.	+0.2	+2.0	—0.8
		P.E.	2.6	1.9	2.0
	B	Diff.	+4.0	+1.0	+7.8
		P.E.	1.2	1.3	1.2

FIG. 3

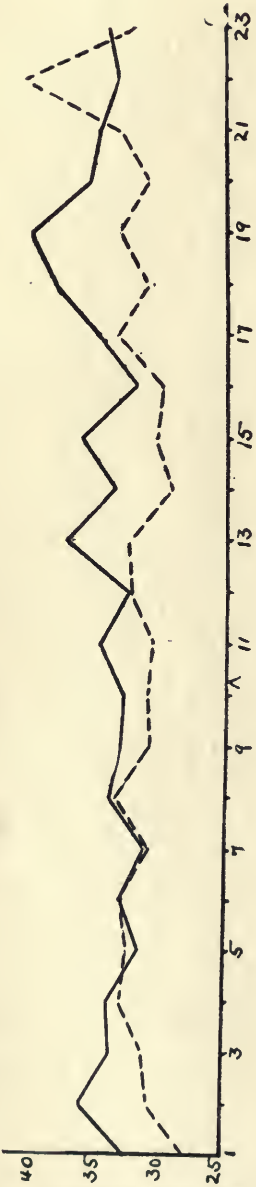

FIG. 4

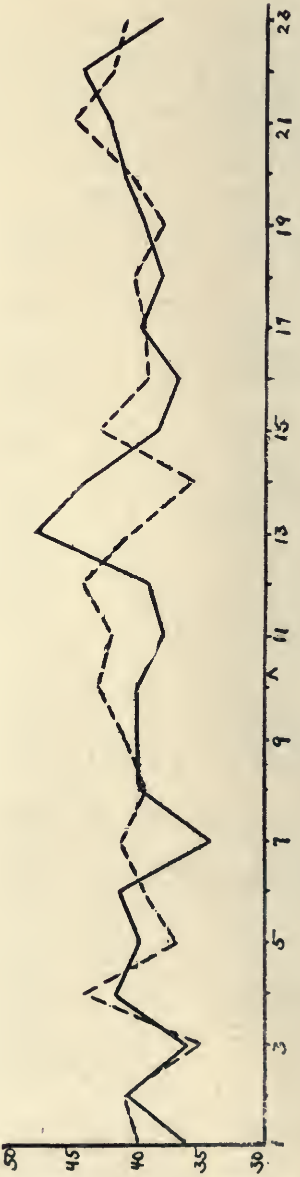

FIG. 3—Curves showing the individual Records of Subject A in the Color-Naming test in the Two Days' Intensive Experiment.

FIG. 4—Curves showing the individual Records of Subject B in the Color-Naming test in the Two Days' Intensive Experiment.

(The broken lines represent the control day and the solid lines the strychnine days.)

group is faster than the second, in three cases there is no difference and in seven cases the first group is slower. Comparing Group III with Group II we find that in eight cases the last group is faster, in one case there is no difference and in seven cases the last group is slower. Hence it is evident that there is no consistent difference among the three groups which would indicate an excitation followed by a depression as a result of the dose of strychnine.

The Cancellation test still remains as one case in which large differences occur in both subjects. It is indeed difficult to understand what function there is which should be so affected in this test and yet which is not included in the other tests.

Figures 3 and 4 show graphically the individual records in the Color-Naming test for subjects *A* and *B* respectively. In each case the *broken line* represents the *control day* and the *solid line* the *strychnine day*. The ordinates show the time in seconds and the abscissas the number of the test period. The carat between period 10 and 11 shows the time at which the capsule was taken. The dose, however, would not become effective until after the eleventh test period. In Figure 3, the curves for A intertwine throughout their course showing no effect from the strychnine. In Figure 4 which represents Subject *B*, the strychnine curve rises above the control curve, representing graphically what was stated in connection with Table IV, that the strychnine time for *B* was slower than the control time. However, inspection of the curve shows that this rise commences several tests before the dose was taken, and hence it must be due to some other influence. This curve reminds us that all the differences appearing in the averages must not be interpreted as purely strychnine effect without constantly referring back to the original data.

It was hoped that in this experiment a condition of fatigue might be reached before the dose was taken, so that the effect of strychnine upon a lowered efficiency might be observed. For the use of strychnine in medicine suggests that it is valuable in raising a lowered activity to its normal, where its influence would be more evident than in stimulating a normal activity to a higher efficiency. In the curves this fatigue would be indicated by a gradual rise of the curve until about the twelfth test period, where a more or less rapid fall would appear. Inspection of the curves however shows that fatigue was not produced by the first eleven test periods.

This intensive study of two subjects supports the conclusion of Experiment A, that strychnine in therapeutic doses does not produce a heightened efficiency in either the mental or motor functions tested, nor is there any depression noticeable within the time limits of the experiment. The apparent retardation in the Cancellation test occurring in the data of both subjects, and in the case of one subject in two of the other tests, is to be attributed to the influence of other factors rather than that of strychnine.

IV. INTROSPECTIVE RECORDS

The "health book," which each subject kept during Experiments A and B, contained the daily record of his condition with reference to the quality and quantity of sleep, headaches, unusual feelings and any introspections which he chose to record, such as suspicion of having taken strychnine on a certain day, and the like. These records show little that can be attributed to the action of the drug.

Of seven nights in which sleep was reported poor by Subject *A,* five followed strychnine days and two followed control days. This subject took a total of 13 doses of strychnine. Of five nights sleep reported below the average by Subject *B,* three followed strychnine days and two followed control days. This subject took a total of fifteen doses of strychnine. Of four headaches reported by *A,* two were in the afternoon of strychnine days and one in the morning following a strychnine day. Of two headaches reported by *B* both were in the afternoon of strychnine days. There were other isolated reports by both subjects concerning peculiar feelings, such as a queer feeling of contraction in the throat, peculiar feelings in the head such as tenderness, dizziness when the head was moved suddenly, feelings of uncertainty about one's movements, feelings of faintness, sharp pains in the legs, difficulty in fixating the eyes, twitchings in different muscles, etc. But these symptoms did not occur consistently and in many cases they appeared only once or twice during the whole experiment.

During the two days' intensive experiment, it is interesting to note that both subjects on their *strychnine day* reported extreme irritability increasing rapidly toward evening, slight headache, and general disgust with the experiment. On the *control day,* however, both subjects report just the opposite. Neither subject felt tired or irritable and each would have been willing to continue the tests over a longer period. At the close of the strychnine day the idea of doing one more test would have been painful. After the two days' experiment was finished, each subject concluded that his strychnine day must have been the one on which the work seemed easy and during which there was no fatigue or irritability. But the opposite was the case.

No conclusions are to be drawn from these introspective reports except the negative one that the strychnine produced no marked effect on the character of sleep, nor any constant physical symptoms. In the two days' intensive experiment the strong feelings of fatigue and general irritability accompanying strychnine doses are to be noted. Probably the most significant point in these records is the lack of any disturbances of the general health, from whatever cause, which would complicate the results.

V. Experiment C

a. Description of Tests and Procedure

In the investigation thus far described, two defects may be pointed out. First, it is not safe to draw conclusions from the records of only two subjects. For the persons chosen might be peculiar in that they were not susceptible to small doses of strychnine. However, it may be noted that the experimental work that has been done with strychnine heretofore and whose results have not been questioned, has been done on only one or two subjects. Second, the test with which all previous work has been done, the fatigue test with the ergograph, was not incorporated as a check in the present experiment.

Experiment C was devised as a check upon these two possible defects. Seven subjects, to be known as *A, B, C, D, E, F,* and *G,* all graduate students in the Department of Psychology at Columbia University, took part. (Subjects *A* and *B* are the individuals who acted as subjects in Experiments A and B.)

The experiment consisted of two half-days of 10 test periods each. The first test was given at 1 P. M. and the others followed at half hour intervals until 6 P. M. Each subject had one strychnine day and one control day, and the same methods were used in concealing the nature of the dose as in Experiment A. The capsule was given at 2.15 P. M., so that it would become effective before the 3 o'clock test period. Thus there were 4 test periods before the dose and 6 after the strychnine became effective. The subjects differ considerably in weight, so that it seemed advisable to give doses of 1/30 grain or 1/20 grain, dividing the subjects into two classes, light and heavy. The schedule of doses, showing the size of the dose and whether the strychnine day was the first or the second of the two, is given below.

Subject	*Strychnine Day*	*Dose*
A	1st	1/20
B	2nd	1/20
C	1st	1/30
D	1st	1/20
E	2nd	1/20
F	2nd	1/20
G	2nd	1/30

None of the subjects except *A* and *B* had any special preliminary practice for this experiment. For this reason, the Color-Naming test and the Tapping test were selected from the list of mental and motor tests, since these are least affected by practice. These two tests were conducted exactly as in Experiments A and B. (Subjects *A* and *B* did not take part in these tests.) Besides these two tests three other measurements were made at each test period, the pulse rate, blood pressure and an ergogram. The pulse-rate was taken at the wrist by palpation and the figures in the table show the rate per minute. The blood pressure was measured with the Tycos Sphygmomanometer by the auscultatory method. Hence the results in the table are in terms of millimeters of mercury. The ergogram was made with the Cattell ergograph. In the ergograms made by previous experimenters, the Mosso ergograph or some modification of it has been used, in which a weight takes the place of the spring of the Cattell ergograph. With the weight type of apparatus, an ergogram consists of the total height the weight is lifted until it can no longer be moved. Such complete exhaustion with the spring type requires hours, so that some limit must be set in making ergograms at frequent intervals. In this case an ergogram consisted of 100 contractions of the index finger at the rate of 2 per second. This rate of contraction was used to induce fatigue as rapidly as possible. But at this rate every subject could make at least 500 contractions, so that the records are not exactly comparable with the complete fatigue records of the other investigators. The ergograms were recorded on smoked paper and the height of each contraction measured. The figures in the table, being differences between control and strychnine days, are calculated from the total distance in millimeters that the finger moved in making each ergogram.

b. Discussion of Results

The results of Experiment C are given in Tables VII, VIII, IX and X. As in Experiment B, the test periods are divided into two parts, those before the dose and those after the dose. The test periods after the dose are then further divided into two groups of three test periods each. Group I represents from one-half to 2 hours after the dose and Group II from 2 to 4 hours after the dose. The figures are derived exactly as in Table V, that is, they are the average differences between the control and strychnine days for each test period.

TABLE VII

EXPERIMENT C

Group Differences Between Strychnine and Control Days

(The columns marked *Before Dose* contain the averages of the differences between the strychnine and the control day, in the first four test periods. *Group I* includes the first three test periods after the dose and *Group II* the second three test periods after the dose.)

Subject		COLOR-NAMING			TAPPING		
		Before Dose	*After Dose*		*Before Dose*	*After Dose*	
			Group I	*Group II*		*Group I*	*Group II*
C	Av. Diff......	+8.2	+7.4	+6.5	+3.2	+0.7	+4.1
	P.E..........	2.5	3.2	0.9	1.2	0.8	2.6
D	Av. Diff......	+5.1	+0.0	+1.3	+11.6	+9.7	+5.2
	P.E..........	0.7	0.2	1.7	1.6	0.4	1.6
E	Av. Diff......	+0.4	—5.0	—1.6	+1.8	+3.5	+2.9
	P.E..........	0.9	2.3	0.8	0.5	0.2	0.7
F	Av. Diff......	—3.0	—2.3	—6.1	—7.8	—5.1	—3.5
	P.E..........	1.7	1.2	0.5	0.8	0.6	2.3
G	Av. Diff......	—8.8	—6.9	—1.5	—3.8	—0.3	+0.3
	P.E..........	2.2	1.2	1.3	1.2	2.9	1.5

Table VII gives the results in the Color-Naming and Tapping tests for the five subjects, *C, D, E, F,* and *G*. An inspection of the data of the Color-Naming test shows that the effects of practice in these untrained subjects must be taken into account. The plus signs in the data of subjects *C* and *D,* who took strychnine on the first day, and the predominance of minus signs in the case of the other subjects indicates that the subjects had a tendency to be faster on the second day than on the first. This practice effect cannot be eliminated in the individual cases, as the experiment was conducted, but by taking an average of all the subjects, including those that had strychnine on the first day and those that had it on the second day, the practice effect can be reduced. It cannot be completely eliminated because there were three subjects whose strychnine day was the second and two for whom it was the first day. Thus a tendency for the strychnine average to be faster could be attributed to this remaining practice effect. The averages so calculated in the Color-Naming test are:

Before Dose	*After Dose*	
	Group I	*Group II*
+0.4	—1.4	—0.3

And from these figures we can see that the first group after the dose was 1.8 seconds faster on the strychnine day and the second group 0.7 seconds faster. The probable errors for these figures are high. Comparing these values with those for the Color-Naming test in Experiment A, we find that *A* was 2.1 seconds slower and *B* was 0.8 seconds slower on strychnine days. In Experiment B, Table III, the strychnine time is longer by about the same amount. Taking the individual records for Color-Naming in Table VII, we find that the values for *C* do not exceed the P.E. Subject *D* apparently increases in speed after the dose, but inspection of the original data shows that the seeming increase in speed after the dose is due to the high positive value before the dose, and this is due to the very long times of the first few tests on the first day. After these first few tests the times remain fairly regular. Instead of a marked increase of speed after the dose, the result is due to very slow speed in the beginning of the experiment from lack of practice. *E* shows an increase in speed after the dose of 5.4 seconds; but the P.E. for Group I is high, so that the reliability of the difference is not great. The values for *F* and *G* are to be explained in the same manner. Especially in the case of *G*, there is a gradual increase in speed from 72.2 seconds in the first test to 53.2 seconds in the next to the last test. Since the strychnine day was the second one, that would account for the high negative value before the dose with the reduction in speed after the dose. When all these facts are considered, no strychnine effect is to be attributed to the figures. As this part of the experiment was intended merely as a check on the previous work, the averages given above, in which the practice effect is partly eliminated, are of most importance. These averages show differences contrary to Experiments A and B and too small to be significant.

The same conclusions must be drawn from the Tapping test records. If the values for the five subjects are averaged, we get the following values:

Before Dose	*After Dose*	
	Group I	*Group II*
+1.0	+1.7	+1.8

Here again the differences are small and the values for the two groups after the dose are practically the same.

TABLE VIII

EXPERIMENT C

Differences in Pulse Rate and Blood Pressure Between Strychnine and Control Days

(The test periods are grouped as in table VI. The figures represent the averages of the differences in pulse rate and blood pressure between the strychnine day and the control day. A minus sign means that the pulse-rate or blood pressure was lower on the strychnine day.)

Subject		PULSE-RATE			BLOOD PRESSURE		
		Before Dose	*After Dose*		*Before Dose*	*After Dose*	
			Group I	*Group II*		*Group I*	*Group II*
A	Av. Diff	+17.0	+12.8	+11.0	+4.5	+1.3	+8.7
	P.E ...	2.4	1.6	0.6	1.5	0.8	0.4
B	Av. Diff	—3.5	—7.8	—1.3	—4.0	+10.0	+3.7
	P.E ...	2.1	1.2	0.2	0.0	0.8	0.4
C	Av. Diff	—13.0	—10.3	—14.0	−2.0	+2.0	+6.0
	P.E ...	1.0	1.1	1.6	2.0	1.3	2.0
D	Av. Diff	—5.0	—6.0	+1.7	0.0	+0.7	+6.0
	P.E ...	0.6	1.0	1.1	1.2	1.1	1.3
E	Av. Diff	+1.7	—3.3	—1.0	—8.7	+1.3	—4.0
	P.E ...	3.9	1.8	1.0	2.4	0.4	2.6
F	Av. Diff	+2.0	—2.0	+2.7	+1.0	+0.7	+3.3
	P.E ...	4.0	2.0	2.2	0.6	1.1	0.4
G	Av. Diff	+4.0	+3.0	+2.3	+2.7	—1.3	—0.7
	P.E ...	2.3	1.0	3.2	1.6	1.2	0.4

The measurement of the pulse rate and blood pressure was included in the series of tests, in the hope that the presence or absence of changes in the physiological processes would indicate whether or not the strychnine had really taken effect in each of the subjects. The knee jerk would undoubtedly have been a valuable measure for this purpose, but without rather elaborate apparatus which could not be quickly attached and removed, or without the assistance of one skilled in making the knee jerk test, the results would have been questionable. The pulse rate and blood pressure had the advantage in that they could be measured quickly and accurately.

TABLE IX

Experiment C

Pulse-Rate and Blood Pressure Values for the Two Groups After the Dose After Subtracting from Them the Average Difference Before the Dose

(This table is obtained directly from table VIII, by subtracting the average difference before the dose from each of the two groups after the dose. It shows the effect of the strychnine on each of the tests, the minus sign indicating a lower pulse rate or blood pressure after taking strychnine.)

Subject		Pulse-Rate		Blood Pressure	
		Group I	*Group II*	*Group I*	*Group II*
A	Diff................	—4.2	—6.0	—3.2	+4.2
	P.E................	2.9	2.5	1.7	1.6
B	Diff................	—4.3	+2.2	+14.0	+7.7
	P.E................	2.4	2.1	0.0	0.0
C	Diff................	+2.7	—1.0	+4.0	+8.0
	P.E................	1.5	1.9	2.4	2.8
D	Diff................	-1.0	+6.7	+0.7	+6.0
	P.E................	1.2	1.3	1.6	1.8
E	Diff................	—5.0	—2.7	+10.0	+4.7
	P.E................	4.3	4.0	2.4	3.5
F	Diff................	—4.0	+0.7	—0.3	+2.3
	P.E................	4.5	4.6	1.3	0.7
G	Diff................	—1.0	—1.7	—4.0	—3.4
	P.E................	2.5	3.9	2.0	1.6

Tables VIII and IX give the data for the pulse-rate and blood pressure tests. The figures in VIII are obtained in he same way as those in Table V, that is, the column marked *Before the Dose* contains the average difference between the control and strychnine days for the first four tests before the dose. Group I and Group II show the same for the first hree and second three test periods after the dose. Table IX shows the values for the two groups after the dose in relation o the value before the dose, or after the difference before he dose has been subtracted from them. The figures are derived from VIII, just as Table VI has been derived from V. These figures would then show the strychnine effect, a minus sign indicating that the pulse rate was slower and that the blood pressure was lower on a strychnine day than on a conrol day.

An inspection of the pulse-rate records in these two tables shows that in the group immediately after the dose was taken (½ to 2 hours afterward), the pulse-rate of every subject but one, Subject *C*, was reduced, that is, shows a minus value. In the second group (2 to 4 hours after the dose) four of he subjects still retain the slower pulse, while the rate of hree has increased. Subject *C* forms an interesting excepion. His pulse is normally rather rapid, but on his strychnine day the rate in the first test before the dose was 68 which fell gradually during the afternoon to 56, but continued rather strong. On the control day the first test showed a rate of 81 which dropped during the afternoon to 70. Therefore, although the decrease in the pulse-rate in this case is quite remarkable on the strychnine day, it is not revealed in he table on account of the low rate of the first test on the strychnine day. Thus all of the subjects show a reduction n the pulse-rate in the first group after the dose is taken. The values are small, some of them little beyond the P.E. But in the case of pulse records there is no practice element to be considered, so that the direction of the signs and the smaller values are significant, whereas in the tests previously mentioned they would not be significant.

Tables VIII and IX show that four out of the seven subjects have an increase in blood pressure in the first group of tests after the dose of strychnine, and that all but one, Subject *G*, had an increase in the second group after the dose. Subject *G*, who weighed only 94 pounds, had a very weak pulse and low blood pressure, the latter about 95, so that it was very difficult to make accurate measurements, and I consider them the least reliable of all that were taken. Excepting this case then, there are five out of the seven subjects with a higher blood pressure in the first group of tests after the strychnine was taken, and all have higher records in the second group after the strychnine was taken. Of the two who have a lower blood pressure after the dose, Subject *F* is lower in the first group by only 0.3 millimeter; and although Subject *A* shows a much lower record in the first group after the dose, this is due to the very high blood pressure in the tests before the dose on the strychnine day.[16] Here, of course, we do not have the factor of practice to consider.

Comparing this evidence for a reduced pulse rate and increased blood pressure after taking strychnine with the statements given in our introduction concerning the physiological effects of strychnine, we conclude that in all of our subjects there is an indication of the influence of strychnine on the circulation. The regulation of the pulse rate and blood pressure is automatic and the centers of their control are located in the medulla, where experimental work has shown that strychnine has an effect.

The results of the Ergograph test are given in Table X, in the form described in connection with the preceding tables. As the figures represent the amount of work done in a given number of contractions, a minus sign indicates that the performance on a strychnine day was *poorer* than on a control day. By comparing the two groups after the dose with that before the dose, we find the following: Four of the subjects, *B, C, D* and *G,* do less work in the first group after the dose than before, while three, *A, E* and *F,* do more. Four of the subjects, *A, C, D* and *G,* do less work in the second group after the dose than before while three, *B, E* and *F,* do more. Now comparing groups I and II, we find that three subjects, *A, D,* and *F,* do less work in the second group than in the first, one subject, *E,* does the same amount in each, and three, *B, C,* and *G,* do more in the second than in the first group. From this brief survey, it is evident that there is no uniform increase or decrease in efficiency in our ergograph test to be attributed to strychnine effect. I do not think the data warrant any more detailed study than this general survey.

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[16] The following figures show the individual records for the blood pressure on Control and Strychnine days for subject *A*:

Control.	117	120	117	117	115	113	112	117	118
Strychnine.	124	122	118	116	115	118	120	127	126

The blood pressure is higher before the dose on the strychnine day than at any time during the control day. If this high value is eliminated from the first two tests, the blood pressure on the strychnine day after the dose will be found to be higher than that for the same tests on the control day. This may be seen by referring to the blood pressure records for *A* in Table VIII.

TABLE X

EXPERIMENT C. ERGOGRAPH

(The test periods are grouped as in table VI, that is, the tests before the dose forming one group, and those after the dose being divided into two groups. In this table the minus sign means *less* work done on the ergograph, and hence a poorer record.)

Subject		*Before Dose*	*After Dose*	
			Group I	*Group II*
A	Av. Diff	—30.8	—28.4	—83.5
	P.E	46.9	10.6	12.3
B	Av. Diff	—100.0	—168.9	—79.3
	P.E	42.9	2.5	3.7
C	Av. Diff	—64.7	—133.7	—108.7
	P.E	90.2	29.2	40.5
D	Av. Diff	+75.0	—103.7	—204.0
	P.E	18.4	11.1	18.3
E	Av. Diff	—53.3	+45.3	+46.3
	P.E	31.1	8.7	14.4
F	Av. Diff	+6.5	+167.7	+150.3
	P.E	47.1	41.7	12.6
G	Av. Diff	+56.0	—47.3	—32.7
	P.E	52.2	28.1	26.1

The data are not offered as proof that strychnine will not influence the ergograph record. The test was employed in the hope that it might give some evidence that the strychnine had taken effect. The material is not sufficient to warrant any conclusion except the negative one in this particular case.[17] The difference in the type of apparatus would probably be sufficient to explain the absence of results which other investigators have found. The spring ergograph cannot induce a state of complete fatigue in 100 contractions, and as the effect of strychnine is given by Féré and Jones to be an increase in the number of contractions before complete fatigue sets in, rather than an increase in the height of the individual contractions, no change would appear in the first 100 contractions.

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[17] Experience with the ergograph in these tests shows that in order to be used to advantage in such work as is here attempted it demands preliminary practice just as much as any of the other tests used. Such details as the taking of a wrong position in the beginning of the ergogram and the attempted shift of position, the stiffness of the muscles resulting after the first one or two ergograms, the learning of tricks of position or movement all tend to cause troublesome variations in the work of the beginner. The subjects in this case were all unpracticed in ergograph work and it is probably this lack of practice which causes the variations in the values of the different groups, and their high probable errors.

Experiment C serves to check up the results of Experiments A and B, by showing that the same results are obtained from five additional subjects as were obtained from the first two; it serves as an additional check by showing, in the case of the pulse rate and blood pressure tests, that the strychnine was actually absorbed into the system of all of the subjects and affected the physiological processes controlling the pulse rate and blood pressure.

VI. Conclusion

The conclusion to be drawn from these three series of experiments is a negative one, and one that is contrary to common opinion, which is, that strychnine has a stimulating effect followed by a period of depression. However, it is not contrary to the results of the previous researches which I have been able to find. The tests previously employed have been only those which have measured the rate of onset of fatigue and that in a purely motor process, the Ergograph test. The present experiments were arranged as a test of general mental and motor efficiency and not as a study of fatigue. Moreover, there was no attempt to determine the influence of more than ordinary therapeutic doses of the drug. What effect extremely large doses of strychnine would have on these processes still remains to be determined.

The tests of motor ability were the Tapping test, Three-Hole test and the Steadiness test. Each of these tests necessitates the coördinated movement or balancing of sets of muscles. No definite effect of the strychnine appears in the results of these tests. The nearest approach to a positive result we find in the Steadiness test, where, although the values are extremely small, the signs are nearly all positive, suggesting a poorer record after taking strychnine.

The tests of mental ability including discrimination, attention and association in varying degrees of difficulty show no effect from strychnine, except in one or two isolated cases, where the result might well be ascribed to other causes uncontrollable in the experiments.

Additional check experiments showed that the two subjects who acted for the major part of the research were not exceptional in their reaction to strychnine, since five other subjects in a short series of tests gave a similar result. Further, the tests of pulse rate and blood pressure showed by the decrease of the former and the increase of the latter after taking the strychnine, that the strychnine must have been absorbed into the system.

Hence, the conclusion is that strychnine in moderate doses taken into the stomach produces no clear cut increase or decrease in efficiency in any of the mental or motor processes studied.

Comparing this conclusion with that of Hollingworth[18] concerning the action of caffeine on these same processes tested in the same manner, we discover the most striking difference. He finds that in the case of the motor tests, small doses of caffeine produced a stimulation in the Tapping and Three-Hole tests, while large doses produced a retardation in the Three-Hole test. Small doses had no definite effect on the Steadiness test, but moderate and large doses produced unsteadiness. In the case of the association tests, Color-Naming, Opposites and Calculation, he finds a stimulation whatever the size of the dose; in the Cancellation test he finds a retardation with small doses and a stimulation with large doses. In no case is there any secondary reaction observed.

A partial explanation of the results of the present investigation and their relation to the caffeine results is offered by Cushny's description of the action of strychnine, caffeine and atropine on the nervous system. (See page 84.) He locates the action of strychnine mainly in the cord and medulla, that of caffeine in the highest or psychic centers of the brain and that of atropine mainly in the centers for coördinated movements. A stimulation by strychnine would tend to produce contractions or twitchings of individual muscles. (This effect was observed in the present experiment, as well as the change in the pulse rate and blood pressure, whose centers are in the medulla.) A stimulation by caffeine would produce a heightened activity of the psychic centers with a confusion of the accompanying mental states, when large doses are given, but no movements would result. A stimulation by atropine would tend to give an excess of coördinated movements such as walking, talking, etc. A further difference between the action of strychnine and caffeine mentioned in the Introduction may be repeated here, namely that in death from strychnine poisoning, the mind remains clear until the end, while caffeine in poisonous doses produces a stupor.

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[18] Loc. cit., pp. 164 ff.

The mental processes then, those which depend on the function of the cortical centers, would not be affected by strychnine. The motor processes which would depend on the center in the brain for the coördination of muscular movements would be affected only in so far as the twitchings and involuntary movements of the muscles would disturb the coördination of muscle groups. These involuntary movements did affect the Steadiness test in some cases, but the twitchings were so infrequent that their influence could be easily hidden by other variations in the subjects' behavior.

Numerous instances have been reported recently where strychnine has been taken by individuals about to enter physical contests and mental tests such as college examinations, in order to increase their ability. In the former case strychnine might be of value in postponing the onset of muscular fatigue. In the latter case, however, where the psychic centers are concerned, and where the processes of association, attention and discrimination are involved, the results of this research would indicate that strychnine is of no value. The influence of suggestion where such a powerful drug as strychnine is concerned is tremendous, and the writer believes that if this suggestion factor were eliminated, those mental operations and all other processes where popular opinion attributes such power to strychnine, would not be affected either one way or the other by ordinary doses of strychnine.

A SIMPLIFICATION OF THE PRACTICE OF THE METHOD OF CONSTANT STIMULI*

By Samuel W. Fernberger, Clark University

The history of the development of the method of constant stimuli has been one of exceeding interest. Starting with Fechner's method of right and wrong cases, it has now become standardized in a form that would hardly be recognised by one not acquainted with that development. The effort throughout has been toward greater exactitude, and the development has been along theoretical mathematical lines,—each advance, however, being verified by psychological experimentation. We now are able to handle the three categories of judgments, which introspection has shown to be the psychological facts.[1] Urban's notion of the psychometric functions[2] has done much to clear up our ideas about all of the psychophysical methods, but especially those of the method of constant stimuli.

Besides striving for greater exactitude and a better theoretical understanding of these methods, there has been a continuous effort to reduce and systematize the calculations required for the determination of the thresholds, with which all of the psychophysical methods are primarily concerned. In the very best and most systematized form of procedure of this method, the time required for the mere calculation of each threshold is from one and a quarter to one and half hours.[3] Such a period is too long to allow the method to be used for anything but a scientific study; it absolutely precludes its use for either anthropometric or diagnostic purposes. In fact few investigators have been willing to undertake the toil of such calculations in spite of the great exactitude of the method and notwithstanding certain other obvious experimental advantages of this over the other psychophysical methods. Of these, the most important is that, with this method alone, the subject is kept in ignorance of the objective relations of the stimuli. The data required for the determination of the sensitivity of the subject consist in a number of empirical observations of the relative frequencies of the various judgments on several comparison stimuli equidistant from one another. From these data it is possible, by an application of the method of least squares, to calculate two constants—h and c—which determine the form and position of the curve representing the psychometric function for the different judgments. Once these values have been obtained it is possible to calculate the value of the threshold, which is defined as that point in the curve of the psychometric functions for the less or greater judgments, where the curve assumes a value one-half, or a probability of 0.5 that this judgment will be given. It is also possible to calculate, by means of these constants, the probability of a heavier or lighter judgment for any intensity of the comparison stimulus. The probability of obtaining an equality judgment is found by subtracting from unity the sum of the probabilities of the heavier and lighter judgments, as these are probabilities of mutually exclusive events. Hence the calculation of the constants h and c for the greater and less judgments determines the course and position of the curves of the psychometric functions for all three categories of judgment.

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* The experimental work that forms the basis of this paper was performed at the University of Pennsylvania in 1912. I am indebted to Professors F. M. Urban and J. W. Baird for many helpful suggestions offered in the preparation of the paper.

[1] Cf. E. B. Titchener, *Experimental Psychology*. II. Pt. 2, 1905. 286ff.

[2] F. M. Urban, *The Application of Statistical Methods to the Problems of Psychophysics*. Philadelphia, 1908. 106ff.

[3] Cf. S. W. Fernberger, On the relation of the methods of just perceptible differences and constant stimuli. *Psychol. Rev. Monographs*, XIV, No. 4. (Whole No. 61), 1913, 29-38.

Such a calculation, however, requires the calculation of the quantities $[P]$, $[xP]$, $[xxP]$, $[\gamma P]$ and $[x\gamma P]$,—values required for setting up the normal equations; and furthermore, two values $[sP]$ and $[xsP]$ which are needed to check up the other five values.[4] This process of setting up and solving the normal equations is very laborious. Even with the help of an adding machine it requires approximately one and a quarter hours; and the use of large multiplication tables and of logarithms is still more cumbersome.

One cause of this great expenditure of time is the size of the numbers that enter into the calculations. The x in every case represents the intensity of the comparison stimulus, for example, in the case of our experiments with lifted weights, 84, 88, 92 . . . 108 grams. Hence one is here obliged to handle two- and three-place numbers in a long and tedious, even if not difficult, series of multiplications. Wirth[5] reduced this calculation considerably by reducing the size of the values of the term x. He reasoned that inasmuch as the values of x were always chosen equidistant, we may substitute the numbers 1, 2, 3 . . . n for these, without affecting the final values of the threshold. Furthermore, as one uses this series of digits in regular order, let us consider the central intensity of the comparison stimuli to be zero; and then for the stimuli smaller, substitute in order –1, –2, –3 . . . –n; and for the larger stimuli, 1, 2, 3 . . . n. In this manner, the values of the term x are greatly reduced. Urban[6] argues that, in this case, the values of x always remain the same, so that it is a simple matter to calculate all of the values for these five products for all possible frequencies of the judgment. This he does; and the tables which he obtains cover only the space of two pages and are therefore very handy and easily applied. In using these tables, one only has to look up the values of the products for the various observed relative frequencies and add each group, thus obtaining the quantities necessary for setting up the normal equations. Also, as the values in the tables are absolutely exact, it is no longer necessary to prove the correctness of the multiplications; and one only has to be sure that they have been transcribed accurately. With the aid of these tables, the calculation for a threshold takes from 15 to 20 minutes or even less.

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[4] For a detailed description of this form of calculation *cf.* Fernberger, *ibid.*, p. 29-38.

It would seem then, that the work of the actual calculations has been reduced as much as possible. He must solve the normal equations, and any reduction must be in obtaining the quantities necessary for the setting up of these equations. It is impossible to see how this may be further reduced, beyond the tables of Urban's, mentioned above, as these require the mere transcribing of the appropriate values. Any further advance, therefore, must be in the line of reducing the amount of work required for making the observations. This may be done either by an improvement in the technique of experimentation or by reducing the number of comparison stimuli used. The question how many comparison stimuli should be used has not been satisfactorily settled. Of course, it would be desirable to use as many as possible; but the time as well as the energy of the investigators have their limitations. It seems that the question was decided chiefly by convenience. The usual number for experimental studies has been seven pairs.[7] Theoretically it should be possible to use two appropriately chosen comparison stimuli, because the empirical determination of two points furnish two equations for the calculation of *h* and *c*, and should determine the course and position of the entire curve of the psychometric functions. For obvious practical reasons such an experimental procedure would be unwise; if for no other reason than that it would be impossible to keep the subject in ignorance of the objective relation in the stimuli presented.

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[5] W. Wirth, *Psychophysik*, Leipzig, 1912. p. 213.

[6] F. M. Urban, Hilfstabellen für die Konstanzmethode. *Arch. f. d. ges. Psychol.*, XXIV, 1912, 236ff.

In a former study, mentioned above, six pairs of comparison stimuli were employed, the seventh being an adjustable stimulus for the accumulation of data by the method of just perceptible differences.[8] A standard weight of 100 grams was the first member of each pair of stimuli. The second members, or the comparison stimuli, weighed 84, 88, 92, 96, 104 and 108 grams. The stimulus for the method of just perceptible differences replaced the usual 100 gram weight of this series. With these stimuli a series of 1,400 judgments were taken upon each of the pairs, two subjects being employed. For convenience, the results for each subject were divided into 14 groups of 100 each in the order in which they were taken.

An examination of the observed relative frequencies[9] shows that for both subjects, particularly after some little practice, the judgment 'lighter' is given upon the 84 gram weight, and the judgment 'heavier' upon the 108 gram stimulus with a very high relative frequency. Hence it seemed reasonable that these extreme values of the comparison stimuli might be disregarded without materially affecting the course of the curves of the psychometric functions. In this case the constants *h* and *c* which determine this course and which are also the values required in the calculation of the thresholds would not be materially affected.

As a result of this, we recalculated the data of our former study disregarding the two extreme values of the comparison stimuli, using only the four central terms,—namely 88, 92, 96 and 104 grams. This calculation was very simple, since the five products for each intensity of the comparison stimulus had been previously determined and their correctness proven. So all that was necessary was to obtain the sum of P, xP, xxP, γP, and xγP for the four central values, substitute these in the normal equations and solve for *h* and *c*. The quantities [P], [xP], [xxP], [γP] and [**x**γP] assumed very different values from the former calculation; but as these are not significant they may well be omitted from this text.

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[7] L. J. Martin and G. E. Müller, *Zur Analyse der Unterschiedsempfindlichkeit,* 1899, 6.

F. M. Urban, *The Application of Statistical Methods to the Problems of Psychophysics,* 1908, 5ff.

S. W. Fernberger, On the relation of the methods of just perceptible differences and constant stimuli. *Psychol. Rev. Monographs,* XIV, No. 4, 6ff.

[8] Fernberger, *ibid,* pp. 6-15.

[9] Fernberger, *ibid,* pp. 20-21.

In tables I-IV following will be found the values assumed by *h, c* and the thresholds S for the lighter and heavier judgments for both subjects and for the original and the recalculated series. Tables I and II give respectively the values for the lighter and heavier judgments for subject I; tables III and IV give the same values for subject II. The tables all show a similar arrangement. In the first columns are found the numbers of the groups of 100 judgments in the order in which they were taken. In the next two columns are found the values of *h;* the first of these being of the original series with the six comparison weights and the next shows similar values for the recalculated series in which only the four central intensities of the comparison stimulus were considered. In the next two columns are found, in a like manner, the original and recalculated values of *c;* and the last

TABLE I

Series	h_1		c_1		S_1	
	Original	Recalculated	Original	Recalculated	Original	Recalculated
I	0.11051	0.10881	10.337	10.172	94.54	93.90
II	0.10769	0.11007	10.108	10.323	93.86	93.78
III	0.11267	0.13983	10.617	13.134	94.23	93.92
IV	0.10976	0.13866	10.442	13.211	95.13	95.27
V	0.11714	0.12113	10.988	11.330	93.81	93.54
VI	0.10432	0.09497	9.814	8.911	94.08	93.84
VII	0.11156	0.10983	10.586	10.435	94.89	95.01
VIII	0.11379	0.11728	10.796	11.096	94.88	94.62
IX	0.11477	0.11727	10.889	11.136	94.88	94.96
X	0.12274	0.11852	11.790	11.382	96.06	96.04
XI	0.09902	0.09253	9.351	8.718	94.43	94.22
XII	0.12287	0.12361	11.670	11.742	94.99	95.00
XIII	0.12794	0.12794	12.251	12.251	95.76	95.76
XIV	0.12446	0.13276	11.795	12.562	94.77	94.62

two columns show a similar arrangement of the values assumed by S_1 and by S_2 or the threshold in the direction of decrease and increase respectively.

TABLE II

Series	h_2		c_2		S_2	
	Original	Recalculated	Original	Recalculated	Original	Recalculated
I	0.11371	0.11264	11.374	11.295	100.03	100.28
II	0.11914	0.12153	11.888	12.114	99.78	99.68
III	0.11629	0.11917	11.537	11.801	99.21	99.03
IV	0.11734	0.13062	11.731	13.043	99.98	99.86
V	0.13496	0.14688	13.406	14.555	99.33	99.10
VI	0.11059	0.10519	11.004	10.510	99.50	99.92
VII	0.11393	0.13854	11.362	13.772	99.72	99.41
VIII	0.11641	0.11936	11.560	11.832	99.30	99.14
IX	0.13843	0.14136	13.724	14.054	99.14	99.42
X	0.13364	0.18659	13.351	18.520	99.90	99.26
XI	0.12086	0.12239	12.081	12.257	99.95	100.14
XII	0.12004	0.12401	12.012	12.388	100.07	99.89
XIII	0.14643	0.13761	14.725	13.916	100.56	100.91
XIV	0.12390	0.15294	12.421	15.202	100.25	99.40

TABLE III

Series	h_1		c_1		S_1	
	Original	Recalculated	Original	Recalculated	Original	Recalculated
I	0.07570	0.06768	7.023	6.266	92.77	92.59
II	0.09224	0.08544	8.728	8.074	94.62	94.50
III	0.10859	0.12446	10.338	11.816	95.22	94.94
IV	0.13786	0.14169	13.018	13.356	94.42	94.26
V	0.10672	0.08403	10.087	7.981	94.52	94.98
VI	0.13459	0.13459	12.587	12.587	93.52	93.52
VII	0.10743	0.11189	10.070	10.455	93.74	93.44
VIII	0.12512	0.12343	11.604	11.428	92.73	92.59
IX	0.12402	0.12918	11.511	11.981	92.81	92.75
X	0.11502	0.11286	10.797	10.563	93.87	93.60
XI	0.10223	0.10223	9.460	9.460	92.53	92.53
XII	0.11750	0.12038	11.126	11.384	94.70	94.58
XIII	0.13071	0.13337	12.307	12.569	94.16	94.24
XIV	0.12920	0.12841	12.239	12.158	94.73	94.68

TABLE IV

Series	h_1		c_1		S_2	
	Original	Recalculated	Original	Recalculated	Original	Recalculated
I	0.08126	0.09794	7.936	9.538	97.67	97.38
II	0.09763	0.10472	9.706	10.402	99.42	99.33
III	0.10666	0.11693	10.500	11.483	98.45	98.21
IV	0.12340	0.14405	12.108	14.106	98.12	97.92
V	0.12580	0.12949	12.291	12.635	97.70	97.57
VI	0.14124	0.16361	13.812	15.913	97.80	97.26
VII	0.11233	0.11980	11.090	11.788	98.72	98.40
VIII	0.11112	0.12129	10.937	11.880	98.43	97.94
IX	0.12023	0.12279	11.816	12.062	98.28	98.24
X	0.10467	0.10708	10.328	10.556	98.67	98.58
XI	0.12377	0.11961	12.161	11.771	98.25	98.41
XII	0.11801	0.11673	11.865	11.737	100.54	100.55
XIII	0.10686	0.09516	10.722	9.634	100.34	101.24
XIV	0.13650	0.14695	13.648	14.649	99.98	99.69

An examination of these tables shows that, although the values of the *h* and S in the two calculations are by no means identical, still, on the whole, they show a rather close resemblance to one another. The course of the values of the *h* for both subjects show a very similar curve for the recalculated series as they do in the original. Hence we would have been able to point out the effects of progressive practice quite as readily if we had considered only the four central values.[10]

But a psychophysical method is primarily a prescription for collecting data and for evaluating them in such a way that the result enables us to compare the sensitivity of different subjects, or of the same subject at different times, or under different conditions. Hence the thresholds are the important quantities in any psychophysical method; and therefore, they are the values which must come in for our greatest consideration. The difference between the averages of the thresholds of the two series is found in no case to exceed one-fifth of a gram. The averages are:

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[10] Fernberger, *ibid,* pp. 41-46.

		S_1	S_2
Subject I	Original	94.736	99.766
	Recalculated	94.606	99.674
	Difference	0.130	0.092
Subject II	Original	93.880	98.741
	Recalculated	93.800	98.623
	Difference	0.080	0.118

These differences are of such little significance that they may practically be disregarded. When we examine the measure of sensitivity $\left(\frac{S_2 - S_1}{2}\right)$ we find a very surprising uniformity indeed. These values are:

Subject I	Original	2.51
	Recalculated	2.53
Subject II	Original	2.43
	Recalculated	2.41

Here we find an agreement which is much closer than might have been expected considering that when we eliminate the data of the 108 and 84 gram weights, we are disregarding one-third of the material that is the basis of this experimental study.

When we consider the individual values that the threshold assumes in the groups of 100 reactions, we find, however, a very different state of affairs and one which does not by any means show the uniformity pointed out above. These values vary from identity (Subject I, S_1, XIII) to a difference of 0.90 grams (Subject II, S_2, XIII). These differences are of course due to variations of a chance character in the observed relative frequencies on one or both of the stimuli which we have disregarded. From the averages of these same values, however, it would seem that in an extended study these variations tend to cancel one another so that the aggregate thresholds and the measure of sensitivity show extremely little variation.

When we consider the form of the curves of the psychometric functions for the heavier and lighter judgments, it is evident that the central part of the curve is the most important.[11] There is a very sharp rise at the central values of our comparison stimuli; while at the ends of the curve, the rise is very gradual—towards unity in one direction and approaching the abscissa asymptotically in the other. Hence these extreme values will not exert as much influence on the value of the constants, which determine the form and position of these curves, as will the central values where the rise is very rapid; and so they may be more readily disregarded.

There is another very practical experimental argument in favor of the elimination of the extreme intensities of the comparison stimuli. It has been pointed out that in any experimental series, no matter how carefully planned, there are certain stimuli which serve as landmarks and which lead to a more or less unconscious learning of the order of presentation of the stimuli.[12] The basis of the method of constant stimuli is that the subject has no knowledge of the objective relation of the stimuli presented to him. To obviate this learning of the experimental order, the procedure has been to change it from time to time when the subject reports or gives evidences of having acquired a knowledge of such land-marks. In the study referred to above, such land-marks were always found to be either the 84 or 108 gram weights—the extreme stimuli. Hence the elimination of these would make it much more difficult for the subject to acquire any knowledge of the objective relation of the stimuli and so, in this case, we should have the method in its purest form. A study of the tables will further support this contention. It will be noticed that, on the whole, the values of h in the recalculated series, where the extreme stimuli were omitted, are greater than in the original series. This matter was referred to Professor F. M. Urban of the University of Pennsylvania and the following is a quotation from a personal letter from him. "On general grounds one ought to expect that omitting the results for 84 and 108 grams might just as well diminish as increase the final result. As a matter of fact this is not so. The explanation may be as follows. The addition of the extreme comparison weights has a tendency to reduce the values of *h*. This they could not do unless the probabilities on these extreme weights were smaller (or larger) than one would expect from the results in the middle part of the series of stimuli. In other words, the extreme comparison weights are not judged in the same way as the others. This is a further argument in favor of omitting the extreme values, since they tend to make the conditions less uniform."

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[11] F. M. Urban, The method of constant stimuli and its generalizations. *Psychol. Rev.,* XVII, 1910. 229-259.
Fernberger, *ibid,* 29f.

[12] Fernberger, *ibid,* 12.

Hence it is evident that there are very sound theoretical and practical arguments in favor of the elimination of the extreme stimuli in the method of constant stimuli in the form under consideration. The calculations show that such a shortening of the series makes, in an extended experiment, practically no difference in the measure of sensitivity of the subject, although the point of subjective equality is somewhat, although not considerably, changed. In short series there prove to be relatively great and by no means regular variations. But it is doubtful if the method of constant stimuli itself may be successfully applied in a short series, inasmuch as the influence of progressive practice changes the final values to a considerable extent. Hence we may consider this method as one which should properly be applied only as an extended experimental procedure; and under these conditions it would seem that the extreme values of the comparison stimuli may be eliminated without materially affecting the final result. Such an elimination not only shortens the calculations to some extent but,—what is obviously of much greater importance,—it reduces the time and labor that must be expended in the acquiring of the data upon which the calculations are based by practically one-third, without, formally at least, affecting the accuracy of the determinations of the sensitivity of the subject.

BOOK REVIEWS

Der Kluge Hans and the Elberfeld Horses

(1.) *Clever Hans, the Horse of Mr. Von Osten;* a contribution to experimental animal and human psychology by Oscar Pfungst, with an introduction by Prof. C. Stumpf, one illustration and fifteen figures, translated from the German by Carl L. Rahn, Fellow in Psychology in the University of Chicago, with a prefatory note by James R. Angell, Professor of Psychology in the University of Chicago. New York. Henry Holt and Co., 1911. Pp. vii, 274.

(1.) In the comparative psychology of horses everything dates from Pfungst's study of *der kluge Hans,* first published in full in 1907 and reviewed in this *Journal,* Vol. XIX, p. 422. The case was that of a stallion which seemed to have acquired, as the result of patient teaching by methods similar to those used in primary schools, a fairly human mental outfit, including among other things a very considerable comprehension of language both spoken and written and powers of abstract thought sufficient for arithmetical calculations. The reports of his performances excited great popular interest in Berlin and other German cities, and aroused no small controversy. He was examined first by a large general commission of distinguished gentlemen of Berlin and finally by a small committee of experimental psychologists of which Prof. Stumpf of the University of Berlin was the most distinguished member, and Dr. Oscar Pfungst of his laboratory the active investigator. The result of Pfungst's study was what appeared to be a complete demonstration that the creature had no unusual powers in language and did no abstract thinking but responded wholly to unconscious movements on the part of the questioner. The work was eminently worth translating, not only as a classic of animal psychology, but for the valuable cross-lights which it throws upon many occult phenomena of human experience. Mr. Rahn's translation is admirably idiomatic and readable, but suffers perhaps a trifle from an excess of this virtue in that the renderings are occasionally a trifle too free. The most serious error which the reviewer has noticed, however, is probably a mere slip of the pen. On p. 145, line 7, the translation reads "with knowledge" and the original (p. 103, line 4, from the foot of the page) "without knowledge."

(2.) *Denkende Tiere, Beiträge zur Tierseelenkunde auf Grund eigener Versuche,* von Karl Krall. Der Kluge Hans und meine Pferde Muhamed und Zarif. 2te unveränderte Auflage mit Abbildungen nach eigenen Aufnahmen. Leipzig, 1912. Verlag von Friederich Engelmann. Pp. v, 538.

(2.) The results of Pfungst were so convincing, even when published in abstract in 1904, that popular interest in Hans fell away entirely, and when Pfungst's book appeared in 1907 the supporters of the opposite view were, for the time at least, effectually silenced. The owner of Hans, Herr von Osten, was not convinced, however, nor was Herr Krall, of Elberfeld, who had followed the controversy in the newspapers and had visited Berlin in the summer of 1905 to see the horse for himself. What he saw and the experiments which he himself made with Hans at intervals during the next three or four years settled him firmly in the conviction that, in spite of all scientific applause Pfungst was wrong and that Hans definitely did his own thinking. Later Krall purchased two Arabian stallions, Muhamed and Zarif, trained them himself by improved methods and secured phenomena far more striking than any obtained with Hans. These results, together with other matters bearing on the general question he has now embodied in the work before us and thus reopened the whole controversy.

It is inevitable that most psychologists, especially those who have devoted themselves to animal psychology, should appear among the skeptical or even among the opponents of Krall's views, but no one can examine the book that he has produced without feeling profoundly indebted to him for the care with which he has gathered all obtainable information about Hans, for the lavish illustrations by which he makes the reader almost a spectator of what he describes, and for his costly experiments which must remain a contribution of lasting value to human and animal psychology, whatever may be the ultimate fate of his interpretation of their outcome. His work is and will remain a "document" of great importance.

Krall presents his material in five main sections. The first (83 p.) deals briefly with the history of Hans and at greater length with tests of his senses and intelligence carried out by Krall in co-operation with von Osten. The second (88 pp.) reports Krall's work with Muhamed and Zarif, his methods of teaching, the supposed spontaneous attempts of the horses at communication and the efforts of the experimenters to facilitate them, and finally with the results of experiments made without knowledge on the part of the questioner (or of others present) as to the answers which ought to be given by the horses. The third part (74 pp.) gathers up the experience of Krall and others on horse education, particularly as regards responsiveness and temperament, with an added discussion on the extent to which animals comprehend and can make use of vocal speech. Following this section comes a statement of conclusions and upwards of 30 pages of painstaking notes touching matters treated in the previous sections. The fourth and fifth sections are appendices and handle in greater detail the subjects considered in the first two sections. The fourth (171 pp.) gives the history of von Osten's efforts in horse education begun in 1888 with an earlier Hans and recommenced in 1900 with the Hans known to fame. From the year 1904 when the later Hans came to public notice the chronicle is as full as industry and a quasi-religious devotion can make it. Extracts from the protocol of Krall's experiments with Hans fill the last 30 pages. The fifth section (50 pp.) gives a similar, though briefer, history of the training of Muhamed and Zarif, accompanied also by transcripts from the protocol. Twenty odd pages of notes again, a full index and a reproduction of several pages of manuscript in the hand of von Osten, conclude the work. Just before the index the author announces in cryptogram, after the manner of the earlier scientists, certain results which constitute the basis for work upon which he is still engaged. The book carries as frontispiece a heliotype portrait of von Osten, and in the course of the work there are two other portraits, besides many cuts of experiments in progress in which he appears as a participant. Besides these there are eight full-page plates and over 150 illustrations, mostly half-tone reproductions from photographs of the author's own taking.

An adequate account of the contents of these sections would require much more space than is here allowable, but it may be said in general that Krall is evidently interested in no small measure in the ethical and other general implications of his findings, and it may be safely conjectured that he is more interested in getting results than in submitting them to critical analysis and confirmation. It is significant that though he made more than 12,000 separate tests with Hans, and probably has made many more than that number with his own horses, there are no statistical tables in his book, nor do the extracts from his protocols give any evidence that his experiments were arranged like those of Pfungst to test the effect of the presence or absence of special conditions.

A brief account of the more striking feats of Krall's horses and a summary of his rather unconvincing arguments against Pfungst's first conclusion—the dependence of the horse upon his questioner—will be found in the article upon Hans and the Elberfeld Horses in the body of this number of the *Journal*. It will be sufficient to say here that Krall seems to make out a pretty good case against the control of the horses by unconscious minimal movements serving as visual signals. As the affair now stands science has no certain explanation to offer; nor is it required to offer any explanation in advance of facts which make some one explanation inevitable. Krall has produced a magnificent problem, but, in the reviewer's judgment, no solution of it.

(3.) *Les Chavaux savants d'Elberfeld,* par M. Ed. Claparède. Archives de Psychologie, XII, 1912. 261-304.

(3.) To most psychologists a report of the first-hand observations upon the Elberfeld horses by a psychologist of Claparède's standing will carry great weight. Such a report forms the core of the article before us. Claparède was present at four seances with the horses in August, 1912, and was so far impressed as to place on formal record his conviction (p. 303) that correct answers were given by the horses under conditions which exclude absolutely the hypothesis of voluntary or involuntary signs and his impression that the horses both really counted the taps given and spelled on their own account, though he reserves his decision as to the manner in which they reach so rapidly the results of their mathematical calculations.

What Claperède himself saw concurs in the main—in what might be called the standard performances of the horses—with what is reported by Krall. He describes his general impression as at first unfavorable and even at the end somewhat mixed. When, in reply to a request that the horses be given some very simple problems, he is told that simple problems bore them since they have become accustomed to more complex ones, he is left in much doubt. Why should not the horses be willing, for the sake of the carrots for which they work, to bear a trifle of ennui? But if they were intentionally controlled why should there be hesitation to submit them to simple tests?

In the latter part of his paper Claparède reviews the hypotheses which he regards as possible, five in number: 1, Fraud; 2, Involuntary signals; 3, Telepathy or an unknown sense; 4, True intellectual powers; 5, A combined hypothesis. The first three and the last of these he rejects, the third and fifth summarily. The fourth he regards as at least logically tenable and makes something of a plea for it, though pointing out that what we need now is facts and rigid experiments.

(4.) *Encore les chevaux d'Elberfeld,* par M. Ed. Claparède, avec une note de M. le Dr. J. de Modzelewski, Archives de Psychologie, XIII, 1913, 244-284.

(4.) The first section of this second paper of Claparède's on the Elberfeld horses is devoted to the results of a second visit to Krall's stables, in March of the current year, made in company with Drs. Modzelewski and Weber-Bauler of Geneva. Claparède had, with these gentlemen, opportunity to work with the horses both when Krall was absent from the city and when he was present and actively co-operating. The results obtained were entirely negative; that is, the horses failed to respond correctly in most of the tests, the percentage of right cases, according to tables given in Dr. de Modzelewski's note, running only from 7½ to 13%. In the few tests made "without knowledge" there were also no successes.

The general failure of the horses Krall thought might be due to their shedding their coats—"always a trying time for them" (p. 250); but we learn from a letter cited by Claparède (p. 257) that two of the horses very soon after worked well for another visitor, and again on p. 267 we find this note by Krall upon Claparède's first visit: "Prof. Claparède was here some time ago and obtained no results. . . . The day after the departure of M. Claparède, an elderly and very friendly major came to see the horses. He succeeded in becoming the friend of the animals, and they worked without making errors. . . . You see that everything depends on the visitors, especially if they are, or are not, on good terms (*sympathiques*) with the horses." We fear that this is the handwriting on the wall for Prof. Claparède at Elberfeld and indeed for any visitor who cherishes scientific reserves.

The second section of Claparède's paper is devoted to a convenient critical review of recent literature arranged according to the hypotheses favored—trickery, unconscious signals, telepathy, and independent intelligence of the horses. The third section is a counterblast to the "protestation" issued by opponents of Krall's views among German zoologists and comparative psychologists.

The final section is the note of Dr. de Modzelewski in which he favors a telepathic explanation—or, as he prefers to term it, the suggestion of motor inhibition (*suggestion motrice d'inhibition*)—as against the hypotheses of fraud, unconscious signals, and independent intelligence. This, to the reviewer's mind, is not far from saying that the horses are controlled by signals conveyed in a manner not yet determined.

(5.) *I Cavalli pensanti di Elberfeld,* by Dr. William Mackenzie. Da una Conferenza tenuta al VI Congresso della Societa Italiana per il Progresso delle Sienze. Genova, Ottobre, 1912. Estratto della Revista di Psichologia, Anno VIII, n. 6, 1912. Pp. 43.

(5.) In Dr. Mackenzie's presentation of the case for the independent thinking of the Elberfeld horses the reader who has traversed Krall's own volume will find little that is new, save a few paragraphs with reference to the personality of Krall (pp. 9 ff.) and brief descriptions here and there of experiments, some of later date than Krall's book and others made in the presence of Mackenzie himself, including a few with the blind stallion, Berto (p. 39).

Mackenzie's standpoint is indicated pretty well by the following conjecture which he quotes with approval: "All that lives around us probably lives *as* we live, and the phenomena of our minds, even those believed to be specific and characteristic, are only a manifestation, different in degree, perhaps, but not different in principle from that of other like phenomena which are revealed to us little by little from remote provinces of universal nature" (p. 7). A somewhat uncritical attitude is, perhaps, betrayed by an occasional loose statement, by the acceptance of at least one very doubtful experiment of Krall's as valid, and especially by the following argument brought forward near the end of the paper (p. 42). Urging that the mathematical performances of the horses ought not to be held improbable because such feats would be difficult or impossible for most human beings, he continues: "But it is not to be forgotten that numberless organisms of the earth and water solve daily mathematical problems just as abstract; I mention only the classical cells of the bee. The supposed circumstance that these organisms neither know nor comprehend what they do, does not seem to me to be the thing precisely which would help our understanding of the enigma." But what sort of a parallel is there here? Is it not contended that the Elberfeld horses are conscious in their arithmetical work? Have they not been taught like human beings and are they not supposed to have grasped the number system? Or if this is not so, and they give their results, as the bee shapes her cell, without knowledge or comprehension of what they are about, what ground have we for crediting them with mathematical intelligence? May we not with equal justification credit them with intelligence in matters of chemistry and physiology because they digest their food?

(6.) *Der Streit um die rechnenden Pferde,* Vortrag, gehalten am 27 Februar, 1913, in der Psychologischen Gesellschaft in München von Dr. MAX ETTLINGER. Mit einem Anhang: Die gemeinsame Protesterklärung auf dem internationalen Zoologenkongress. Verlag Natur und Kultur, München. S. 54.

(6.) If the views of Krall find in Claparède a judge inclined to mercy and in Mackenzie an aggressive champion, they meet in Ettlinger a hostile and experienced critic. Ettlinger is a literary man and magazine editor of Munich, a student and writer of some years' standing in animal psychology, and one of the conservatives in the original Hans controversy.

His critique, to his regret, lacks one important feature—a first-hand study of the horses—but like a number of others of openly critical attitude, he has not been able to secure the chance for such a study under satisfactory conditions. His account of the horses and his estimates of the reliability of the results are therefore drawn of necessity from the printed sources and the personal reports of those who have been in this respect more fortunate than he.

After a brief resumé of the work of Pfungst, he adduces confirmatory evidence from other sources, especially from the practice of professional showmen, and then considers the performances of Krall's horses, giving chief though not exclusive attention to their mathematical feats. Ettlinger's contention is, of course, that Muhamed and Zarif are guided by visual or other signals given consciously or unconsciously by persons within sensory range of the horses, and that the precautions which Krall and others have employed to exclude such guidance, have been insufficient. He lays especial stress on the wholly inadequate use of experiments "without knowledge."

Perhaps the portion of Ettlinger's discussion which will seem most interesting to the average reader is that in which he shows how easy it is for one with very moderate mathematical skill, aided by a very little special knowledge, to solve mentally examples in the extraction of roots which at first sight seem of so great difficulty that they have been employed at Elberfeld as experiments "without knowledge" on the ground that the answers were unknown to the spectators and could not be reached by any one present as promptly as they were gotten by the horses. Such an example as $\sqrt[5]{4084101}$ seems to the uninitiated a fairly embarrassing one for head reckoning, but when it is known that the last digit of any exact 5th power is always the same as the last digit of the root (and therefore in this case 1) and that the tens in the root are to be found from the figures remaining at the left, when the five at the right have been pointed off, here 40, it is easy to see that the root required can only be 21. The case is a little more complicated for cube roots, though here again the final digit of the power always bears an easily determinable relation to the final digit of the root. The errors made by the horses with examples in cube and fifth roots are also such as to support the hypothesis that the roots in both cases were obtained by estimate under guidance of knowledge of these special relations and not by actual calculation. Square roots and fourth roots offer much greater difficulty and appear not to have been so much used as the odd roots and when used to have been tried in a good many cases with relatively easy examples.

Of considerable interest also is the citation from the Düsseldorf veterinarian, Dr. Karl Wigge, who reports that at least in one instance he saw an opening and closing of the eyes of the groom coincident with the beginning and ending of a set of taps given by Muhamed, and that when on another occasion he could place himself in such a position that he could see the groom from head to foot the horses did not succeed at all. Wigge and others have likewise noted, as was observed also in the case of Hans, that the horses seem to pay little or no attention to the blackboard from which they are supposed to be reading or on which their problems have been written, from which the inference is easy that they are not reading or calculating at all.

And so the matter stands, with much assertion upon one side and much denial on the other, waiting for the crucial experiment which, however, is not likely to be made unless Herr Krall is willing to offer more facilities than he has yet been ready to offer to those whom he knows to be hostile to his views. E. C. S.

Die Realisierung. Ein Beitrag zur Grundlegung der Realwissenshaften. Band I. By OSWALD KÜLPE. S. Hirzel, Leipzig, 1912, pp. VII, + 257.

The problem of *Realisierung*, as set forth in this book, is the problem of apprehending in and through experience that which truly exists or has existed (p. 3). Professor Külpe states further that his purpose is to provide a justification for a realistic theory of objectivity, and he announces that his plan contemplates the publication of four volumes, of which the present one is the first. In this introductory volume the author limits himself in the main to a discussion and refutation of views which conflict with his realistic position. These views constitute two main types, which he designates respectively as *Konscientialismus* and objective idealism. Of these the former maintains that all reality is such only in relation to consciousness, whether this consciousnes be viewed as a particular fact or as consciousness *überhaupt*. It denies the possibility of dealing with reality independent of consciousness; or, in the language of the author, it refuses to admit that a process of *Realisierung* actually occurs in human knowledge. It maintains that the real is identical with content of consciousness. On the other hand, objective idealism grants that the real is not content of consciousness, but something to be achieved through intellectual endeavor. Its emphasis, however, is placed, not on the real as existent, but on the process; the real is not presupposed, but construed; investigation is a process of world-creation into merely asymptotic approximation to an endlessly remote goal.

Nearly the whole of the volume is devoted to a discussion of the first of these alternatives, for the reason that, in the opinion of the author, the results of this discussion make it possible to dispose of the idealistic contention with great brevity. The consideration of *Konscientialismus* is introduced with a careful and elaborate statement of its fundamental doctrines, which are then reviewed and refuted in detail. The central thesis of the argument is that the facts of consciousness are not reality but the datum which constitutes our point of departure in the attainment of the real. In other words, the real is not a matter of direct apprehension, but is attainable only as an object of thought (p. 240). The error of *Konscientialismus,* so it seems, consists, on the one hand, in the neglect of unperceived psychical contents and the difference between sense-objects and the objects of science, and, on the other hand, in its failure to appreciate the radical difference between sense-impression and thought. As Professor Külpe phrases it, the qualities presented in sense-impression are necessarily contents of sense-impression, but objects presented to thought are not in the same manner contents of thought. Impressions of color are necessarily impressions, but we can not say similarly that the thinking of an object reduces the object to a thought.

While avoiding personal polemics, the author treats the insufficiencies of *Konscientialismus* with much dialectical skill and with considerable freshness of presentation. The inadequacy of the appeal to immediate awareness, the ambiguity of solipsism, the logical admissibility of an extra mental object, the impossibility of reducing the objects of science to data of consciousness, the fundamental difference between sense and thought, are all set forth with commendable clearness and impartiality. Having thus defended his view against this enemy, he turns to a brief exposition of the likeness and differences between his standpoint and that of objective idealism. The two are in agreement in recognizing a real which transcends the data of consciousness and in constituting it an object of thought. The author is apparently not wholly averse to the notion of a pure, creative thought (pp. 250-251), but protests against the exaggerations of idealism. Unless we view our experiences as clues or indices to pre-existent fact in the realistic sense, we are tempted in the direction of an *a priori Naturphilosophie,* and are left without a standard or criterion by which to control the results of our thinking.

These remarks will perhaps serve to indicate Professor Külpe's mode of approach and the problems that rise up in his path for consideration in the later volumes. Sense-impression and thought are widely sundered, and the task of *Realisierung* is a task for thought alone. The author does not permit himself to suppose that this first volume has accomplished more than a refutation of the objections which might be urged from other points of view. The problems and difficulties inherent in the position will no doubt stand out more clearly when the attempt is made to demonstrate the internal coherence of the standpoint. It may be permissible, however, to suggest a difficulty that appears to be peculiarly formidable. If sense and thought are so widely divergent as we are led to believe, the suggestion lies close to hand that thought must necessarily have categories of its own, in which case it would seem to be in a position to claim *a priori* validity for the world of reality to which it refers. The author's doctrine of thought is faithful to the realistic presuppositions of this standpoint in that it postulates a world upon which thought operates more or less *ab extra,* in the sense that this external world does not participate in the processes by which the knowledge of it is achieved. Hence the assertion that there are unperceived psychic processes,—whatever that may mean; hence also the hard and fast separation between concepts and their objects (cf. pp. 230, 234). Unless the author is able to bridge over this gulf between sense and thought in some novel way hitherto unsuspected by philosophers, his defense of "realism" necessarily remains inconclusive. The reduction of the individual to a bundle of universals (pp. 137, 215) and the concessions made to idealism with regard to the nature of thought, give ground for the suspicion that Professor Külpe's realism is an insecure super-structure reared on a foundation of objective idealism.

The book should be of special interest to American readers in that it furnishes an able discussion of a topic which is prominent in our current philosophy. It is supposed to clear the ground by showing that the objections to a realistic view of objectivity or *Realisierung* are inadmissible. The American reader, however, can not fail to be struck by the omission of all reference to pragmatism from the discussion. That a movement of such significance, vigor and scope, with its novel and cardinal doctrine of *Realisierung,* should fail even of polite mention by a writer like Professor Külpe is another illustration of the fact that to our German friends a *Würdigung* of the philosophic activities of America is a waste of time. According to pragmatism the philosophy of Professor Külpe shares with the *Konscientialismus* and objective idealism which he attacks a conception of the relation between thought and object which makes it possible for refutation and counter-refutation to go on without end, and which makes the hope for a coherent doctrine of *Realisierung* on that basis a delusion. What is needed is, so it contends, a thorough-going reconsideration along lines which it indicates, of the nature and function of thought and of the criterion of objectivity. Whether the pragmatic doctrine of *Realisierung* be right or wrong, the omission of this alternative leaves ground for the claim that Professor Külpe's conclusion in favor of realism begs the question at issue, and it seriously impairs the value of an able and readable book.

University of Illinois. B. H. BODE.

An Experiment in Linear Space Perception. By Francis Norton Maxfield. Psychological Review Monographs, XV (whole No. 64), 1913. pp. 56.

The purpose of the experimenter has been to apply in another sense field, the statistical methods outlined in F. M. Urban's treatise. (The Application of Statistical Methods to the Problems of Psychophysics, 1908.) Dr. Maxfield works in the field of visual perception and, more particularly, the perception of small differences in the length of lines. His material consisted of a series of seven pairs of lines; each pair composed of a standard line 150 printer's points long, and a comparison stimulus from the series 144, 146, 148, 150, 152, 154, and 156 points long. These were exposed on a rotating drum, and in every case a standard and comparison stimulus were exposed simultaneously; the standard being always to the left. The time of exposure was approximately 1½ seconds. Great care was taken to keep the objective conditions constant. Judgments were given on each pair of stimuli in terms of the three categories "equal," "longer" and "shorter." In all, twelve subjects were used and 3,500 judgments were obtained from each of them.

These records enable the experimenter to obtain the absolute and relative frequencies with which the various categories occur for the different comparison stimuli. Dr. Maxfield applies the coefficient of divergence of Lexis and Dormoy in order to ascertain to what extent the conditions influencing the judgment given remain constant throughout the experiment. He obtains in every case a fairly large supranormal dispersion which indicates changes in these conditions. This, the author believes, may be explained by the fact that none of the subjects had any training in the technique of this particular experiment and, indeed, half of them had never had any training in a psychological laboratory. In fact he finds that the coefficients for the group of untrained subjects are very much larger than for the subjects who had had former psychological training.

By interpolation according to Lagrange's formula, the author next calculates certain intermediate values of the probabilities with which we may expect our three categories of judgment on the values of the comparison stimulus between those which have been empirically determined. He then constructs the curves for the psychometric functions for all twelve subjects, and the article includes the graphs for all of them. These graphs show clearly the great individual differences in the subjects.

The next consideration is the relation of the method of just perceptible differences to these empirical results. The author obtains two series of values of the thresholds. One series was calculated on the basis of the probabilities of the different judgments; and the other was derived from observed values of the just perceptible and just imperceptible positive and negative differences. There is a close correspondence between the values of the interval of uncertainty which were found by interpolation, and those which were calculated by the method of just perceptible differences. A comparison between the observed and theoretical values of the method of just perceptible differences shows less correspondence, due probably to great variations among the different subjects in sensitivity to the objective relations of the stimuli.

The rest of the paper is devoted to a consideration of the equality judgments. Dr. Maxfield finds a marked regularity in the ordering of his subjects, when he compares the value of the probable error with the value of the maximum probability of the equality judgments. When dealing with the second of these his subjects fall into an order which is almost the exact reciprocal of their order by the value of the probable error. Other serial arrangements are made, as in Urban's Monograph, on the basis of the length of the interval of uncertainty by the method of just perceptible difference and by interpolation, the probable error of the mean of the equality judgments, and the probable error of a single equality determination. Some degree of regularity is apparent in these comparisons.

This study is extremely interesting for the reason that it shows that Urban's notions as regards the psychometric functions, and all the notions derived from that concept, are valid in another sense field than that of lifted weights. The experimental arrangement is ingenious and should be applicable in the future to many experiments in the visual field. The author's presentation of his material is very clear and concise.

Clark University. Samuel W. Fernberger.

On the Relation of the Methods of Just Perceptible Differences and Constant Stimuli. By Samuel W. Fernberger, Ph. D. Psychological Review Monograph Series, XIV, No. 4, 1913. pp. 81. Price $1.

The formal character of the two methods having been shown to be identical there remains only the problem of the variations due to the experimental procedure. Fernberger's conclusions are based on lifted weight experiments which were conducted so that the space error was eliminated and the time error was constant in the first order. Seven comparison weights were used, six of which, for the method of constant stimuli, were constant in intensity and the seventh, for the method of just perceptible difference, was variable. No information was allowed the subject in reference to the weight lifted so that there could be no anticipation as to the outcome of the judgment. In the results obtained by the method of constant stimuli the effect of practice is clearly seen. Fernberger calculated the coefficient of divergence and finds a slightly over-normal dispersion which shows that the conditions did not remain absolutely constant. Since the experimental arrangement was such that a change in the objective conditions was very unlikely we must suppose that a change in the psychophysical constitution of the subject took place. Through the course of the curves plotted for the value h, in the successive series, the author is able to conclude that the variation in the distribution of the judgments is due to the state of practice of the subject. Then he shows, by a comparison of these results with results obtained from one of his subjects in a previous investigation, that this practice effect is not due to a definite training of the sensory apparatus involved but to a more consistent concentration of the attention on the task. In the method of just perceptible difference an important element is the attitude of the subject when he knows he is going through a descending or an ascending series. The use of alternate series and their presentation simultaneously with the series for the method of constant stimuli tended to neutralize the errors due to this condition of the subject.

A comparison of the two methods is the main issue of the article. It has been shown by Urban "that the common source of both methods is to be found in the notion of the probability of a certain judgment" and it is obvious that there is no essential difference in the individual judgments since they are given on the basis of the relation of two stimuli presented in the same way. The comparison of the methods must then be made on the basis of the results obtained, the important results for the comparison being the measures of sensitivity, that is, the upper and lower thresholds. In these values there are found the expected unsystematic variations due to limited experimental data, the variations in the results for the method of just perceptible difference being the greater since these are the less extensive. But besides these chance variations there is a constant difference in the results of the two methods. The thresholds in the method of constant stimuli are always closer together than the same values for the method of just perceptible difference. The greatest difference is found in those series where, for the method of just perceptible difference, a large number of small steps were made and the least difference in those series where a few large steps were used. The amount of variation seems to be due to the number and size of the steps and the more satisfactory series is that in which a close approximation is made to the series for the method of constant stimuli. If such a series is used the results of this study seem to indicate that any discrepancies between the results would be so small that they could be disregarded.

University of Pennsylvania. DAVID MITCHELL.

BOOK NOTES

Outline of a study of the self. By Robert M. Yerkes, and Daniel W. LaRue. Cambridge, Harvard University Press, 1913.

The authors tell us that they have discovered as teachers that the study of ancestry, etc., is profitable and hence this outline, which is to help people understand themselves and to become good members of society, to sympathize with others and with children, and to arouse interest in heredity. Those who answer the questions are exhorted to be wholly honest with themselves. The authors' work seems to base on the questionnaires issued by the Eugenics Record Office at Cold Spring Harbor, the blanks of which are published in the few printed pages which constitute this outline. Altogether this medley of blank sheets, eugenic records and notes by Yerkes and LaRue, leaves the reader very much in doubt as to its purpose, place or usefulness. The young man who undertook to answer all these questions in the way the authors desire him to do would be old before he had finished and would have a strange ingrowing type of consciousness, which we should think pathological. Every error to which the questionnaire method has ever been subjected would confront the man who attempted to utilize such data.

The psychology of learning; an experimental investigation of the economy and technique of memory. By E. Meumann. Translated from the 3d edition of "The Economy and Technique of Learning" by John Wallace Baird. New York, Appleton (1913). 393 p.

It is a real service which the translator has performed in putting this third edition, which has been revised as the new work in the field required, into English. We can only mention the seven chief topics treated here: a survey of the modern psychology of memory, its function, observational learning, treated as to the technique of observing and learning and the analysis of observational noting, with an experimental investigation upon it, associative learning, also treated in three chapters, one comprising technique and economy, another memory, learning and ideational types, economical learning; and the third chapter treats learning of significant materials, education of memory in the schools, and actual efficiency compared with what it might be. There are three appendices, on construction of nonsense syllables, the meaning of economy of time and energy in learning, with addenda from the most recent literature.

Worcester State Hospital papers, 1912-1913. Offered in compliment to Dr. Hosea Mason Quinby on the event of his retirement from the superintendency after twenty years of service. Edited by Samuel T. Orton. Published by the Mass. State Board of Insanity, Series 1913. Nos. 4-19 inclusive. 198 p.

This is a very creditable report and we are interested to see that it gives due credit to Dr. Edward Cowles for pioneer work in establishing a laboratory at the Worcester Insane Hospital such as he had developed at the McLean Hospital. Very interesting is Dr. Cotton's study of "Certain Problems of Heredity in Mental Disease," which is perhaps the article that more than any other will interest genetic psychologists. Dr. S. T. Orton himself, the editor of the volume, has an interesting article on "Some Methods for the Routine Examination of the Brain from Cases of Mental Disease." Meyers writes on "The End of Metastatic Tumors" and on "New Formation of Nerve Cells." Orton ends with an interesting article on "The Brain of a Catatonic Hirntod."

History of psychology; a sketch and an interpretation. By JAMES MARK BALDWIN. New York, G. P. Putnam's Sons, 1913. 2 v.

This little sketch is extremely popular and extremely general. The first volume comes down to John Locke, and the second to nearly the present time. The author takes a wide view of the field though not to our mind wide enough. It is easy to criticize, but we ought rather to be grateful to anyone who has had the boldness to enter this field and to say anything of his contemporaries. The last volume contains portraits of Ribot, Fechner, James, Herbert, Spencer, Helmholtz, Lotze, Herbart, Rousseau, de Biran, Condillac and Locke, while the first volume goes farther back and includes Descartes, Bacon, Scotus, St. Augustine, Plato, Socrates, Pythagoras.

Sammlung kleiner Schriften zur Neurosenlehre. von SIGM. FREUD. Leipzig, Franz Deuticke, 1913. Dritte Folge. 321 p.

This work is simply a reprint of ten recent articles of Freud. beginning with the famous analysis of the phobia of a five-year-old child, which was printed in 1909, and ending with the recent essay on psychogenetic disturbances as conceived by psychoanalysis. The important articles are, "Remarks on a case of imperative neurosis," "Psychoanalytic remarks on an autobiographic case of paranoia." "After-thoughts upon the same," "Formulation of the principles of psychic happenings," "On the counter sense of primitive words," "The future chances of psychotherapy," "Wild psychoanalysis," and "Types of neurotic diseases."

Die psychanalytische Methode; eine erfahrungswissenschaftliche-systematische Darstellung. von OSKAR PFISTER. Leipzig, Julius Klinkhardt, 1913. 512 p. (Pādagogium, Band I.)

This is a work that was greatly needed. The first volume, as noted above, is devoted to the psychoanalytic method, wherein the author first discusses the problem, then the theory, and then proceeds in successive sections to discuss in detail repression and fixation, *Rückwirkung* and its relations to repression, and in the second part takes up the technique of psychoanalysis, its methods, the way the psychoanalytic procedure develops, the course of treatment, the prerequisite conditions, the practice, and finally the results. It is a work that is greatly needed and will be welcomed by all interested in psychoanalysis.

Das Unterbewusstsein. Untersuchung über die Verwendbarkeit dieses Begriffes in der Religionspsychologie. von. GEORG WEINGÄRTNER. Mainz, Kirchheim & Co., 1911. 158 p.

These chapters deal with consciousness, ego, personality, the essence and achievements of the subconscious (*Unterbewusstsein*), the proofs of its existence, and especially its relations to religious psychology, which the author believes are profound and constitute its chief merit.

Über den Selbstmord; eine pathologisch-anatomische und gerichtlich-medizinische Studie. von HERMANN PFEIFFER. Jena, Gustav Fisher, 1912. 195 p.

This is an interesting and exhaustive study of the subject down to date, full of statistical tables, pathological, anatomical presentations, study of special cases, generalizations and finally conclusions, the chief of which is that we must strive to keep the human stream clean and pure by elevation of the popular health and education, so as to make the soul of the people as permeable as possible to sources that protect and elevate, and to keep off all degenerative influences which not only cause this self-extinction of many, but very distinct deterioration of very many more who do not actually kill themselves.

Advertising and selling; principles of appeal and response. By HARRY L. HOLLINGWORTH. New York, D. Appleton & Co., 1913. 314 p.

This is by far the best book we have yet upon this subject. It is very comprehensive also, beginning with the nervous basis of mental processes, and dealing with the analysis of task and media, catching attention, mechanical inventions, interest incentives, experimental tests of attention and memory value, of various mechanical devices, holding the attention, the feeling-tone, provoking response, instincts, relative strength, etc. There are scores of very interesting and well chosen advertisements to illustrate the teachings of the book.

Self-realization; an outline of ethics. By HENRY W. WRIGHT. New York, Henry Holt & Co., 1913. 429 p.

The writer first considers ethics as a science of good conduct, and treats its subject matter, methods, problems, the conscience, the appeal to the good or moral obligation and the motive of good conduct. In Part Two he discusses the nature of the good in conduct and character, its theories, hedonism, rationalism, volition, self-realization, and freedom and its conditions. Under Three, Good as Self-realization, he contrasts this with self-sacrifice, motives of goodness, happiness, systems of virtue and finally under Part Four, which is very brief, he discusses the life of self-realization, the individual, social, religious virtues.

The marking system in theory and practice. By I. E. FINKELSTEIN. Baltimore, Warwick & York, 1913. 88 p.

This little work treats first of theoretical considerations as to whether marks should indicate performance or ability of accomplishment, the theoretical distribution of qualities or traits, best method of translating distribution into a scale of symbols; the third chapter treats of distribution of marks at Cornell from 1902 to 1911, and combined results for numerous courses. Chapter four is on the results for individual courses, showing variations produced by change of instructors, high and low markers, etc.

Studien zur Entwickelungsgeschichte der ornamentalen Melopöie. von ROBERT LACH. Leipzig, C. F. Kahnt, 1913. 727+98 p.

This is a very elaborate and scholarly work, beginning with a discussion of the nature of oriental culture races, then passing down to the Ambrosian and Gregorian singing, the Catholic church song. etc. Upon this primary aesthetic motive came the architectonic motive, which supervened and penetrated it in the European folk song and secular music, on to the end of the middle ages. Then followed the development of the new Melopöie in the sixteenth century. Now begins the systematic chief part, child music, animal music, problems of its origin, general theory of its development, with conclusions.

Experimentelle Untersuchungen über die räumlichen Eigenschaften einiger Empfindungsgruppen. von TH. ZIEHEN. (Fortrschritte der Psychologie und Ihrer Anwendungen, hrsg. v. Karl Marbe, I. Band, IV./V. Heft. Leipzig, B. G. Teubner, 1913. p. 227-337.

This is a very comprehensive little treatise with twenty-three chapters, among the chief of which are the original idea of angles and their development, also that of number, the nature of geometrical proof, unconscious processes involved, the position, form, and magnitude, the development of mathematics, psychological considerations, etc.

Linguistic stocks of South American Indians, with distribution-map. By ALEXANDER F. CHAMBERLAIN. (Reprinted from The American Anthropologist, April-June, 1913. Vol. 15, no. 2, p. 236-247.)

Zweck und Gesetz in der Biologie; eine logische Untersuchung. von RICHARD KRONER. Tübingen, J. C. B. Mohr, 1913. 166 p.

This interesting paper discusses, first, metaphysics, then biology, by which the author understands the mechanical view, its relations to Darwinism, vitalism and biologism. Under biology as empirical science he discusses the idea of species, the problem of generation, the theory of knowledge, and the system of natural science, and under organic teleology the writer discusses the outer and inner purposiveness, teleology and doubt, Lamarck and Darwin.

Jesu Persönlichkeit; eine Charakterstudie. Von KARL WEIDEL. 2d enl. ed. Halle, Carl Marhold, 1913. 128 p.

This is a very careful but absolutely free psychological study. The writer is not a theologian but a lay savant who had been penetrated with a sense of the uniqueness of Jesus' character, which he has therefore tried to define from all original sources available.

Antike Schriften über Seelenheilung und Seelenleitung auf ihre Quellen untersucht. von PAUL RABBOW. I. Die Therapie des Zorns. Lpz., B. G. Teubner, 1913. 198 p.

This interesting article goes back to the work of Posidonius and traces the subject down through Plutarch and Seneca, whose elaborate views upon the subject are presented *in extenso.* Then the work of Antiochus and Chrysipp as treated by Cicero are considered. In an appendix we have a study of the definitions of anger and critical notes in regard to the literature on the subject.

Modern problems of biology; lectures delivered at the University of Jena, December, 1912. By CHARLES SEDGWICK MINOT. Philadelphia, P. Blakiston's Son & Co., 1913. 124 p.

These six lectures discuss the new cell doctrine, psycho-morphosis, the doctrine of immortality, the development of death, the determination of sex, the notion of life.

Doctrines held as vitalism. By H. S. JENNINGS. New York, 1913. (Reprinted from The American Naturalist, July, 1913, p. 385-417.)

On formulation in psycho-analysis. By FREDERIC LYMAN WELLS. (Reprinted from the Journal of Abnormal Psychology, Boston, Oct.-Nov., 1913.) 13 p.

A syllabus for the clinical examination of children; with the revised Binet-Simon scale for the measurement of intelligence. By EDMUND B. HUEY. Baltimore, Warwick & York. (1912.) 45 p.

Internationale Zeitschrift für ärztliche Psychoanalyse; hrsg. von SIGM. FREUD. I. Jahrgang, 1913. Heft 3, Mai, Heft 4, Juli, and Heft 5, September. Leipzig, Hugo Heller & Cie.

Verbrechertypen, hrsg. von HANS W. GRUHLE und ALBRECHT WETZEL. I. Heft. Geliebtenmörder, von Albrecht Wetzel und Karl Wilmanns. Berlin, Julius Springer, 1913. 101 p.

Bulletin No. 5. Government Hospital for the Insane, Washington, D. C. Edited by William A. White. Washington, Government Printing Office, 1913. 101 p.

Chippewa music—II. By FRANCES DENSMORE. Smithsonian Institution, Bureau of American Ethnology, Bulletin 53. Washington, Government Printing Office, 1913. 341 p.

L'année pédagogique, publiée par L. CELLÉRIER et L. DUGAS. Deuxième année, 1912. Paris, Félix Alcan, 1913. 524 p.

The principle of mental tests. By FREDERIC LYMAN WELLS. (Reprinted from Science, N. S., Vol. XXXVIII, No. 973, pp. 221-224, August 22, 1913.)

Hypnotismus und Suggestion. von E. TRÖMNER. 2d rev. ed. Leipzig, B. G. Teubner, 1913. 114 p.

Einleitung in die Philosophie. von OSWALD KÜLPE. 6th rev. ed. Leipzig, S. Hirzel, 1913. 376 p.

Travaux de la clinique psychiatrique de l'Université Impériale de Moscou. Sous la direction de TH. RYBAKOW. Moscow, A. E. Mamontov, 1913. 384 p. (Printed in Russian.)

Causes and determiners in radically experimental analysis. By H. S. JENNINGS. New York, 1913. (Reprinted from The American Naturalist, June, 1913. p. 349-360.)

Experimental pathology of the higher mental processes. By F. L. WELLS. (Reprinted from The Psychological Bulletin, vol. 10, no. 6, June 1913. p. 213-224.)

Die "Neue Wirklichkeitslehre" in der amerikanischen Philosophie. von GÜNTHER JACOBY. (Reprinted from Internationale Monatsschrift für Wissenschaft, Kunst und Technik. Okt. 1913, v. 8, nr. 1.) 13 p.

The psychopathology of a case of phobia—a clinical study. By MORTON PRINCE. (Reprinted from The Journal of Abnormal Psychology, Boston, Oct.-Nov. 1913.) 17 p.

Die konträre Sexualempfindung des Weibes in den Vereinigten Staaten von Amerika. von DOUGLAS C. MCMURTRIE. (Reprinted from the Arch. für Krinimalanthropologie und Kriminalistic. Bd. 55. Leipzig, F. C. W. Vogel, 1913. p. 141-147.)

De l'animal a l'enfant. par P. HACHET-SOUPLET. Paris, Félix Alcan, 1913. 176 p.

Le rêve et l'action. Par GABRIEL DROMARD. Paris, Ernest Flammarion, 1913. 368 p.

Die Aktion gegen die Psychologie; eine Abwehr. von KARL MARBE. Leipzig, B. G. Teubner, 1913. 32 p.

Bewusstsein und psychisches Geschehen. Die Phänomene des Unterbewusstseins und ihre Rolle in unserem Geistesleben. von L. LOEWENFELD. Wiesbaden, J. F. Bergmann, 1913. 94 p.

NOTES

The University of Bonn has recently conferred the honorary degree of Master of Liberal Arts and Doctor of Philosophy upon Professor Lillien J. Martin of Stanford University, in recognition of her contributions to experimental psychology and aesthetics. The diploma mentions Dr. Martin's achievements in evolving methods of suggestion and her method of projecting visual images. It also describes her contributions to the psychology of imageless thinking and to the literature of psychological aesthetics.

ANNOUNCEMENT

A prize of One Hundred Dollars ($100) is offered for the best paper on the Availability of Pearson's Formulae for Psychophysics.

The rules for the solution of this problem have been formulated in general terms by William Brown. It is now required (1) to make their formulation specific, and (2) to show how they work out in actual practice. This means that the writer must show the steps to be taken, in the treatment of a complete set of data (*Vollreihe*), for the attainment in every case of a definite result. The calculations should be arranged with a view to practical application, *i.e.,* so that the amount of computation is reduced to a minimum. If the labor of computation can be reduced by new tables, this fact should be pointed out.

The paper must contain samples of numerical calculation; but it is not necessary that the writer have experimental data of his own. In default of new data, those of F. M. Urban's experiments on lifted weights (all seven observers) or those of H. Keller's acoumetrical experiments (all results of one observer in both time-orders) are to be used.

Papers in competition for this prize will be received, not later than December 31st, 1914, by Professor E. B. Titchener, Cornell Heights, Ithaca, N. Y., U. S. A. Such papers are to be marked only with a motto, and are to be accompanied by a sealed envelope, marked with the same motto and containing the name and address of the writer. The prize will be awarded by a committee consisting of Professors William Brown, E. B. Titchener, and F. M. Urban.

The committee will make known the name of the successful competitor on July 1st, 1915. The unsuccessful papers, with the corresponding envelopes, will be destroyed (unless called for by their authors) six months after the publication of the award.

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A SYNTHETIC GENETIC STUDY OF FEAR*

By G. Stanley Hall

CHAPTER I

Fear is the anticipation of pain. For those forms of life capable of fear this anticipation is not prevision but only a highly generalized fore-feeling, itself unpleasant, that a yet more painful state impends. The will to live, the *élan vital,* is more or less checked in its momentum or narrowed in its range by some kind of intimation that it may be still further held up. This protensive or futuristic attitude or orientation toward a pejoristic state is the specific *quale* of the psychic condition called fear. Psychogenetically it is a primitive *Anlage* of futurity and it is the most stimulating and vivid of all its forms of presentation. In fear the future dominates the present and gives it a new significance in addition to its own, and but for fear pain could do little of its prodigious educative work in the animal world. Fear is thus the chief paradigm of psychic prolepsis as well as the chief spur of psychic evolution. The *Einstellung* it motivates to the "what next" and the "about to be" may become, second only to present pain, the most intense of all psychic experiences. This power to fore-feel pain, although not unanalyzable genetically, is nevertheless primary, unitary and unique enough to be considered practically, if not scientifically, as indeed it always has been popularly, as a prime category of the emotional or affective life. In its most generic form it is identical in all stages of animal life, for the function of pain-anticipation is common to and underlies all its innumerable differentiations as to objects, its degrees, physiological concomitants and modes of expression.

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*I have called this study synthetic because in it I have tried to correlate all the chief lines of study of fear, normal and morbid, in children and adults, men and animals. As the synthetic principle I have found geneticism of chief value.

Fear has only one genetic presupposition and that is some experience, individual or racial, up and down the algedonic scale. There must have been suffering and this must have left some trace. Here, then, we have germs of both memory and recollection, however rudimentary and unconscious. From this point of view we may further define fear as revived traces or engrams of past pains in prospect of passing over into re-realization which latter is normally more intense than its merely reproduced forms. If pain had not been felt it could not be anticipated, hence the condition precedent of fear is some kind of registration (whether transmissible by heredity from forbears or individually acquired we shall discuss later) and some degree of revival of these vestiges. Thus fear involves the past as we have seen it does the future. Without conservation of past experiences there could be no fear, nor could there be if the phosphorescence of the traces left by the past were more painful than their dreaded reinstatement itself, which latter case indeed occurs, but only in certain psychalgias in which the pain of the present moment is so excruciating that any presentiment of a greater one is impossible. In a general sense, then, and subject to many specific limitations, we may say that both the intensity and variety of fear depend on the intensity and variety of the pains that have before been felt. Too much suffering tends to timidity, too little exposes to avoidable dangers without either warnings or defenses, and the optimum between these extremes, which varies greatly with individuals, will some day be an attainable and perhaps individually prescribed goal of orthogenesis as it already is of psychotherapy. Not only are herbivora and all creatures being preyed upon timid and carnivora relatively fearless, but many phobias are the direct result of shocks and we may say in general that the first fear in the world could only come after a preceding pain.

As to the relation of fear to the third division of time, the present is never so full of content as at the moment of acute fear. The latter is differentiated sharply from all other times and crammed with psychic activities. All resources are rung up and focused on the now as well as on the here. There is often a unique sense of "it has come" common to other intense, summative experiences. The present seems to swell, stretch, suck up the past and future into itself. Time is bunched, as it were, in a block. We are never so conscious of fractions of a second and they are never so fateful, although we are perhaps never less conscious of abstract, homogeneous time. The soul is perhaps never so vital, awake, so penetrated with the sense of its own existence as well as of its own worth, so desirous of conserving itself, and at the same time it is never more filled with a sense of the reality of the environment. Hence it is never so active and potentialized on the one hand and never so passive and saturated with a feeling of its dependence on the other. All forms of animal life have experienced it, yet it is so intense and absorbing that much self-observation is impossible, introspection knows it not, and the characterizations of it by psychologists show little conception of its nature or importance, but usually consist of a few hackneyed phrases indicating how it feels to be afraid, a description of a few of its physical expressions and the assignment to it of a place in some abstract classificatory scheme. In fact every philosophical category discussed from Aristotle to Hegel and every psychological function are implicit in the simplest state of urgent fear, and it may stimulate or check about every physiological function.

Thus if Bergson's *durée réelle* or pure duration or time freed from spatialization which the intellect always tends to give it, has any existence, it is in the pure psychic state of fear. If pleasure-pain is the result of the first day's work of creative psychic evolution, fear is that of the second. Above the scope of all parallelistic and interaction theories and long before the development of the wide domain where the Lange-James theory applies, we must thus posit a fear state which, when it had once broken through, became an independent existence with a promise and potency of its own as truly as did the vertebrate plan of life when it arose. At bottom and in its most generalized form it is elemental, at least beyond the reach of present psychological analysis. It has great explanatory power but at root is itself inexplicable by any or all of its phenomena. Nothing save pleasure and pain has such invincible psychological reality. No purely psychic factor has such dominance over the body or over health and disease. If we assume a pain state in the lowest organisms which would bring katabolism and dissolution and that this process might pass on to death with no counter tendency to check or to rejuvenate it, such elemental vital units might pass on to extinction and feel no fear. But no such creatures are known to exist for fear has its biological basis as deep as immunization, regeneration of lost parts and recuperative processes generally, including those of resistance of fatigue. We do not then need to assume that the fear state actually exists in isolation from its usual bodily concomitants, *i. e.*, without affecting function or passing over to the motor zones of which it is one of the great organizers, although as we shall see, there are other pathological states that seem to approach this isolation in the receptive stages. The pure fear state then is not unlike a very active but widely diffused chemical element which profoundly modifies animate and inanimate nature, but which, because of its many and intense affinities, we have never yet been able to completely isolate though we may go on indefinitely learning more about it. Philosophers have long ascribed to the pure intellect and poets to love an existence so independent that they could survive the body. But there is probably nothing psychic of which we have so much reason to make any such postulate, were we so disposed, as we are not, as we have concerning the pain state, for no psychic activity has such power to control the soma in all its developmental stages and in all its functions. Hence if there be a vital principle fear must be one of its very close allies as one of the chief springs of mind. Thus if any psychic component is not a mere epiphenomenon, but has an entity of its own, it is this.

In fact fear is intensely *dynamogenic* and also *inhibitive.* The prospect of actual pain puts to life the question of its very survival or extinction, complete or partial. Something bad has begun that is prelusive of something worse, but this worse must if possible be avoided. So feeling must pass over into doing. The narrowing of the pleasure field, or its conversion into its opposite, makes the strongest of all appeals to the efferent tracts to energize to their uttermost. Much conduct, behavior, many habits and even motility itself throughout the animal world might be described as more or less organized pain-fugues which are correlates of the pleasure-tropisms.

Here belong all, perhaps even physiological, protective and defensive phenomena and methods of escape from enemies or the perils of the environment which are essential for survival and are precious because wrought out at great cost and through countless generations. In the life of the individual, and still more in that of the race, the way of orthogenesis is relatively straight and narrow and is the result of more failures than successes in the trial and error processes of evolution. Fear profoundly affects appetite, hunger and every digestive, alimentary, nutritive and excremental process, the secretion of probably all glands, circulation with its attendant thermal changes, the *vita sexualis,* muscle tension and hence all voluntary and involuntary movements. It influences the function of every sense, stimulates or impedes thought and will, intensifies or even paralyzes all activity and even consciousness itself. It may disorganize all usual co-ordinations and adaptations to the environment. Fear always means a new situation which compels to a new adjustment. In new and dangerous situations almost any change helps and the greater the danger the fewer among all possible changes are those that could increase it and the more those that mitigate it. Hence it comes that often the most incoherent and even convulsive movements are, on the whole, beneficial. The greater the sum total of activities within and without, the greater the chance of escaping increments of pain. Since about everything has been feared even to morbid intensity, and since objects of fear are repellant, those animals and men with many fears lead restricted lives beset with limitations and taboos. Timorous creatures sneak, skulk, hide and may become nocturnal, subterranean, and fugitive in their habits, cowardly and deceptive in deed and word. Fearsomeness, too, stimulates the imagination to create fictive objects of dread and terror and much of the great body of degrading superstition is its product, and panics of this weird infection make the soul mad with baseless, craven horror. Fear invades sleep and shapes dreams which are the very opposite of wish-fulfillment. It has invented supernatural terrors for the nursery and a future life full of torment and woe. Much of the work of science has been to expel baseless and superstitious fears and to teach men to fear aright.

Yet fear has its fascinations, and strong, adventurous souls not only face danger when it comes, but go forth to meet it. Cowardice thus has its countervalent impulse in courage. The salt of danger is one of the great appetizers of experience. The prospect of pain acts as a tonic and one does not need to be a hero to love to take risks and to venture in order to have. Not only is one measure of values the dangers we will face to attain them, but curiosity, lust for knowledge, wealth, power, ambition control if they do not cast out fear. The great culture heroes set men free from fears. Without known danger life would be tame, insipid, asthenic. Men fight best if rightly afraid, and even weak animals which would fly when brought to bay fight with the energy of desperation. When the victim of delusions of persecution at last turns and becomes himself a persecutor, he becomes most dangerous, as Magnan has so well shown.

Here then we have illustrations of the law of ambivalence which pervades the entire emotional life. Any emotion may pass over into its opposite, which latter then becomes the stronger because of the preceding antithetical state. The intensifying effects of complementary colors on each other, the well known laws of contrast, the Hegelian logic that proceeds by thesis and antithesis, are themselves, like the above illustrations, only very partial expressions of a far larger and deeper principle. In the first case the anticipation of pain, a state usually avoided as itself unpleasant, is actively desired for its own sake and perhaps because of the augmented sense of vitality it brings which makes us tingle and glow with a feeling that dormant powers in us are aroused. Danger makes us more alive. We so love to strive that we come to love the fear that gives us strength for conflict. Fear is not only something to be escaped from to a place or state of safety but welcomed as an arsenal of augmented strength, and so, instead of dreading, we come to love anticipations of pain. This, however, is only a sub-variant of the more generic case of the rapture of woe discussed in connection with pleasure and pain. The other more important case of it is the whip-row relation of fear that flies and anger that fights. Each of these readily passes over into the other and reinforces it. Even in the extreme of either the other is not entirely absent, so that both fear and rage have in them elements that reinforce the other. We always fear what we fight and would fight what we fear. But this theme we reserve for the chapter on anger.

This brings us to *hope* as the opposite of fear, the anticipation of pleasure as fear is of pain. These two interpenetrate and oscillate hardly less than do pleasure and pain themselves. Wherever there is prospect of a pain increment, there is also one of an increase of pleasure. Hence much, if not most, of what has already been said concerning the prospect of pain may also be said of the prospect of pleasure. The latter also has roots in the future and past but focuses in the present in a peculiar way. The foregleam of coming pleasure has given to life the organs, habits and other means of its attainment and has been a great factor in both the modification and development of both animal and human conduct. In order to hope we must have experienced pleasure and all hope is itself pleasure-toned. It would attain and not escape. If its crucial moments of opportunity are less tense and its impulsions on the whole less urgent and less charged with fateful significance and its instants of abandon to the end sought less pregnant of consequences, it has on the other hand increased in scope with the progress of civilization and the widening of the pleasure field till all now begin to hope for all. Each of these, too, intensifies the other and it is really fear with which the heart is sick if hope is deferred. Each of these, too, may become a diathesis and the balance and proportion of the two have much to do in making up the weal and woe of life. The expectation of pleasure may have all degrees of confidence from the faintest to the most assured, and oscillations up and down the scale may be constant for every object of desire. Hope impels plans for amelioration which the will is to execute. If in human life fear has predominated in the past, hope now increasingly prevails, yet most wild animals know far more fears than hopes. Hope, when freed from the function of reality makes dreams, reveries, illusions, myths and revels in the joy of fancied attainments. In optimistic moments, temperaments, theories and religions it predominates as fear does in those that are pessimistic. If the psychology of fear is superficial and inadequate, that of hope is almost non-existent for intimations of pleasure are too near, vital and all-controlling to be broadly observed or adequately characterized. The balance between these opposites is always tipping in every conscious or unconscious effort, otherwise motivation to action would be lost. Each, too, is the inverse measure of the other as the prospect of attainment alternates with that of failure. Their relation is so ambivalent that neither could exist without the other. Paretic delusions of grandeur are the extreme delusions of hope, as the horrors of delirium tremens or some suicide motives are of fear. The soul has many devices to suppress fear in order that it may the more indulge hope, and if all hope is gone suicide is the only sane and logical refuge of despair. Hope is benign, for it makes for expansion, progress, and new enterprises, while fear is malign because it not only represses but brings retrogressions. Thus whether we call fear an instinct, feeling, emotion or sentiment, we must call hope the same, for each is the affective converse or complement of the other, without which it can neither be understood nor explained, so that a broader knowledge of fear will have to wait upon future studies of hope which is at the same time the light of life and also the *terra incognita* of psychology.

Fear can only be understood genetically. Its reinforcements and physiological expressions are full of atavistic rudiments, for we inherit not so much the effects of specific objects feared as the physical and psychic diathesis of fear. It long precedes the nervous system and its base is found in the very earliest and simplest forms of life. It is preformed by, genetically allied with, and doubtless avails itself of much of the energy of repulsion in the world of physics and chemistry, and this shades up into negative tropisms, taxies and tonuses in which we already see the embryonic *Anlage* of fear. When protozoa like paramoecia react to a stimulus by a fixed formula of motion—reversing the movements of the cilia and going backward, making an aboral turn and then going forward again at another rather constant angle—although these creatures have no life-history and many species of them react in a similar way whatever the stimulus, how or wherever applied, and do so even though the motion may bring them into more rather than less danger—all this shows hardly more psychic quality than mere irritability such as exists in the excised muscle of a frog's leg. Yet the extent and rate of these movements depend on the strength of the stimulus. During the series of sub-efficient stimuli applied to such a muscle so that, *e. g.*, at the tenth, on the average, the muscle contracts, it is assumed that the lability of its molecules is increasing during all the preceding stimuli and that their effects are summated. This physiological anticipation is abundantly proven although we know relatively little of its exact nature. So, when paramoecium is moving toward the center from which an acid is diffusing, we must assume some summative process anticipating the backward movement and turn. In the many similar studies of other protozoa so devised as to show their tendency to find an optimal position between the hot and cold ends of a trough, the right intensity of a solution, color, degree of light, distance from the anode or cathode, or even in some results of experimental botany upon the flagellate spores of certain plants—to say nothing here of phagocites and moving cells in the body—we may indeed regard these reactions as those of either a substance or of an individual, as due to physical or chemical attraction and repulsion and to mere mechanism, or we may assume some anticipatory pain with the antikinesis. We have here at least a matrix of pain anticipation and these movements are often effective as fugues from death or serious injury. These phenomena permit at least a pragmatic, quasi or *als ob* assumption of fear. If an unpleasant thygmo- or hapto-taxic or stereotropic experience were anticipated by ever so rudimentary a visual, chemical, vibrational, thermal or other less unpleasant one, we have a condition for the fear state, as to the actual existence of which in these lowly forms of life we incline to rest not upon the safe and easy, agnostic *periculum credere aut non credere,* but, despite the fear of animism or even anthropomorphism, to emphatically postulate a fear state on the basis of the larger law of continuity. Just as the body, so the soul, is implicit in the foetus or fertilized ovum, although as different from the adult body as from the adult soul. To assume that the psychic element comes in at a certain point is only an attenuated survival of the old creation hypothesis. Let us rather say, then, that it is there from the first though we know not how, for the psychic rudiment is not so much the softest of all soft parts but it is, unlike germinal somatic forms, inaccessible to observation. In the same way, then, as the mature man is implicit in the ovum, so fear is implicit in the above conditions and if we had some kind of microscope that could detect the form or even some litmus paper that could test the presence of things psychic, we should see in such phenomena the real radical of fear. Just as the body is a colony of cells so we are compound minds with certain pontifical kinds of functions as of cells and psychic life is no less a genetic continuum than physical, with no chasms or gaps although with innumerable nodes, checks, accelerations, perhaps saltatory stages, explicable in very various degrees. The above phenomena, although not yet fear as man knows it, constitute its cunabula, or better, are its genetic predecessors. The question between Bethe, Uexküll, Beer, Ziegler, Roux, Loeb, Dantec, Pearl, Ludhoff, Gassey and many others who deny, and Wassmann, Binet, Forel, Verworn, Altmann and others who postulate a psychic element in lower forms of life, stimulating and fruitful as this question has been, is not yet and perhaps never will be answerable in terms of exact scientific demonstration, certainly not until there is some agreement as to what a psychic element is. This question suggests that between those who think phyletic influences are the chief ones in determining the development of the individual and those who, especially since the establishment of Roux's *Archiv für Entwickelungsmechanik der Organismen* in 1895, have been chiefly interested in showing the influence of chemical and physical forces rather than those of heredity as determining the processes of growth (His having even gone so far in his antagonism to Darwin, Haeckel and Weismann as to intimate that the growth of all parts and organs of the body in the embryo are the results of successive foldings so that rudimentary organs are like "the superfluous pieces of cloth that are left over when a coat is cut out even in the most economic fashion," a view which Haeckel dubs the "rag bag theory"). Brilliant and voluminous as are the additions to knowledge made by the mechanists in both the above fields, those in the latter can never dethrone the mighty principle of phylogenesis, although they may make its sway like that of an ever more minutely regulated constitutional monarchy, while the tropists have really given far more than they have taken from psychogenesis.

Among the many conceptions of what the simplest primeval psychic element is like, *sentiency* has perhaps most often been proposed, but such a sentiency must of course be apart from and precede the deliverances of any special sense, even touch, the mother of most of them. Sentience, besides being too highly generalized to have any but a merely verbal definition, implies some kind of objectivity over against a subjectivity and this pair of opposites, which looms so large in the history of thought, cannot be very primordial. Introspection suggests the more intellectualistic *awareness* (*Bewusstheit*). But this is a generalization from the self-observation of mature minds and not an element, even though it be not quite fair to call it an abstraction. It suggests an incipient invagination impressed from without and not an inner state before and independent of any outward reference. It is at best only a noetic element and the impingement it implies is external, so it can only be a rudiment or antechamber of consciousness itself. Many others have suggested *choice* or a voluntaristic element or an act of will deciding between two alternatives or of attention selecting one thing in preference to another. But volition, too, is only one of the functions of the soul although its genetic roots doubtless strike much deeper than those of awareness, although to find anything preluding options we must go below the paramoecia. Again, *memory* in the sense of Hering has been a candidate for the holy motherhood of mind. It is understood as the basis of organized matter itself and so is in some sense deepest and earliest of all. It really means only registration or engramization. The influence of this concept has been not so much to psychologize biology as to biologize psychology. It is used as a principle not only of growth but of heredity, so that even plants remember their creator, the phylum, especially in the days of their youth. It applies to life at least quite as much as to mind and was a most pregnant and useful suggestion but cannot be accepted as the genetic criterion of the psyche.

Hence we must turn to *affectivity* and also go below consciousness and seek something not primarily objective, but the flushing up of a state, and this we find in the pleasure-pain state which seems best to fill the specifications. Whatever moves up and down on this scale is psychic and to do so marks the advent of the first psychic process. With this arose a new type of functioning and also of causation as new as life itself when it arose.

This first Eocene flush of mind was thus not a neutral state between pleasure and pain but one of them—euphoria or dysphoria—if pleasure, more diffused, if pain, more acuminated. In the absence of knowledge we would prefer to assume the former as the first faint sense of being alive. It was a tenuous, present, unanchored feeling good or well, but with no subject to know it and so no glimmer of *what* feels well and with no certain reference. Perhaps, on the other hand, this first phosphorescence of the psyche may have been algic, and the hedonic element later. At any rate these two points on the pleasure-pain scale emerged perhaps at first as separate outcrops in a hitherto apsychic world. But they were near, and the appearance of the junction between them was not long delayed and with or before this came the orientation of each toward the other and the scalar relation of reciprocity between them, one drawing, the other repelling. In some such way we may conceive the seeds of mind were formed and began to grow. Many have thought that the first manifestations of the psyche were far more rank and lush than any now, like vegetation in the carboniferous age or animal life in that of the great extinct saurians compared with their degenerate descendants today. Cope opines that primitive cells were more like brain cells than any other. So, too, the tickle sense with its intense phenomena may be a relic of the vivid nature of experience before touch had tamed down the contact sense to make it serve the practical purposes of life. Experimenters, again, have often noted how insects, crustacea and even lower forms react more quickly to slight stimuli than they do to strong, perhaps injurious, and even lethal, ones. Preyer even thought that the whole inorganic world was dead protoplasm, and studies of radium show how inconceivably greater are intra- than inter-atomic energies. So perhaps the psychic principle, when it first made its advent, was so crammed with potentialities yet to be realized in organized life that it shaped not only habits but organs, took control of the early zoölogical world and made things generally, so that in the field of life more than we dream is due to germinal mind in the heyday of its prime, and thus we may even speak of the work of extinct mind in a sense larger than Lotze used it as having laid down reflexes and various forms of instinct, though this conception is much narrower than that of Schelling who saw in all the ascending orders of nature only dead or dying mind. The opinion is now often expressed that existing lower forms of life, especially those in the line of man's descent, do not fairly represent what their extinct congeners were when they contained in themselves the nisus which has since evolved into higher forms, but are only their still surviving poor relations. On this view ontogenesis is only a series of tiny pools trickling the one into the next in the bed of what once was a mighty torrent. If there ever was such a high tide of crude psychic life it might, before its energies were exhausted in modifying organs and shaping instincts, set back and fill previous possibilities which it had not done before, so that with the fore-and-upward-push of the developmental impulse there was a backwash that increased the psychic function of lower processes already evolved and caused it to break out where it had not appeared before (somewhat as important organs regress to the embryo and appear in earlier stages of development in proportion to the significance they have acquired in mature stages). On this hypothesis highly organized mind has been drawn out and is more or less spent or exhausted because differentiated and teased out into so many specific types of activity, its threads woven into new association textures, with, on the whole, vast gain, but not without much loss of the freshness and vigor it had when it was so homogeneous as to always act as a whole with all that was in it. Then fear, *e. g.*, knew no check, and vented itself with abandon and panic, and flooded or perhaps dammed all the sluices of activity. The power to anticipate pain was an epochful acquisition and gave great survival advantage to creatures who acquired and developed it. It was a state that tingled and glowed and absorbed all available energies so that no other state was possible, and it had every biological process at its mercy. Thus on the base line of the pleasure-pain scale, when it was once laid down, the most vital of all experience was acquired. Of course lives were narrow and only a few things gave pleasure or pain. The neutral stretch between them, too, might be very wide and there were more faint than vivid degrees of both, but each state was easily maximized and it was these that first made primitive life psychic as it oscillated from one to the other or ceased, sinking, when poised between them, beneath the threshold which separated mind from life.

This brings us to *shock*, the specific nature of which, and its relations to fear, have never been adequately understood. Here there is no anticipation of pain and therefore no forefeeling of it, but the pain is sprung without warning. This Spencer makes the psychic beginning. There is no preliminary dis-ease but a detonation of pain, which comes perhaps with fulminating suddenness, with no rudimentary set, *Einstellung*, and with no fore-gleam of expectant attention to prepare for it. Nothing better illustrates psychogenesis than do the reactions to shock, or shows how pervaded psychic life is by paleo-atavistic vestiges. It may find its chief expression in any part or function of the alimentary canal, affecting appetite, mastication, deglutition, secretion of all the glands from mouth to anus, peristalsis, defecation and micturition, may bring all kinds of unwonted sensations, tonuses or relaxations and modifications of every anabolic and katabolic stage and process. It may vent itself in the circulatory system, accelerating, retarding or even arresting the action of the heart, modifying blood-pressure, causing pallor or flushing and affecting even the composition of the blood, and is somehow closely correlated with the activity of the adrenals or their product. Respiration, too, is always affected. The voluntary muscles may be relaxed or tense and there may be tonic or clonic cramps or diffuse convulsibility and " masses of clotted motion " or almost any specific act, attitude, or perhaps paralysis. In the nervous system it may affect any or every sense, either intensifying or almost suppressing its action, may quicken the mental functions or more commonly arrest or disintegrate them into their proximate elements or bring insanity or even amentia, and create morbid complexes, obsessions and phobias galore, causing schizophrenia and even persistent changes in the moral character, or may strike the sexual nature and bring polymorphic perversions, inversions and about every type of arrest. The chief point here to be noticed is that these effects of shock are always more or less reversionary. They strike the weak points and may erupt in any phyletic level, the assimilative, secretive, sympathetic, autonomous, affect the lower or higher spinal, the sub-cortical or cortical centers, but there is always a downward tendency on the phyletic scale. Thus shock and probably fear reactions have no specific center or zone but may occur in any, and the diathesis of vulnerability to them may vary much with individuals and in general; to understand them we must not only know the personal history of the subject but something of the laws of heredity and the history of the race.

Thus the utterly new and sudden is always dangerous because we lack the apparatus to react fitly to it and so lapse to elementary responses and these, like rudimentary organs, are highly variable because long side-tracked and dispensable. Such reactions often act as emergency-functions to relieve inner tension and, like fire-extinguishing devices, may fail to act or break down from long disuse at the critical moment. Thus shock brings out prehistoric and sometimes even embryonic activities. When the synapses that usually transmit from the receptor to expressional spheres are either blocked or overpressed, movements cease to be coherent or purposive and reinforcements, inhibitions, or both, are wild. Had our forbears always had only such experiences as they could meet adequately, higher, lower or louder notes of life might seem harmonious and be danced to aright, and we might be poised and resourceful in more startling exigencies. On the other hand, if the individual and the race had had more intense experiences we might, even without any erethic warming up, have acquired the power to digest more exceptional and unforeseen emergencies. Conversely, much and frequent experience with excessive shock is liable to develop and fix low level and abnormal modes of response, and this sometimes brings deep-seated and persistent dread of reality lest we react to it in ways that ought to be obsolete; and so we blush, tremble, sweat, feel nausea, the impulse to relieve the bowels, grow breathless, show tics, awake senseless phobias and lose presence of mind instead of acting sensibly and with courage. These old primary effects, secretory, vaso-motor, bristling, goose-flesh, etc., are the more intense the lower we go on the animal scale and the less the influence of the cortex, which tends to suppress them, even although some think a few of them may serve some useful purpose by way of kindling or arousing the higher centers to act although these extravasations always detract and waste energy.

This great principle of the reversionary nature of shock applies all the way from plants, which the botanist Ettinghausen showed exhibit *Rückschlag* phenomena if mutilated or if cold, up to the moral relapse to savagery Lombroso and Wagner first pointed out as characteristic of criminals. If the cerebrum with its moderative and directive influence is removed, animals, as Goltz and Bechterew have proven, show very intense symptoms of fear and so do human monsters born without brains, or hemicephalic children, as Sternberg and Lotzko have demonstrated. Many invertebrates respond to shock by immobility or cataplexy, a reaction of great service among lower forms, the enemies of which have imperfect eyes chiefly sensitized to motion and which therefore seem to assume that most which lives moves, and this survives in man in paretic states or weakness. Buckman found certain little twitchy *klammernde* movements in the hands and feet in fright which remained longest in the hands, which he deemed relics of flight during the arboreal life of our ancestors. Mosso's rabbits responded by paling or reddening over the whole or in parts of the ear at a whistle, a bird flying over or even a cloud obscuring the sun. Frightened apes leaping from bough to bough evacuate, not as was once thought, to deter their pursuers below but by way of lightening their bodies, expressing their emotion by peristalsis, sphyncter relaxation, or both. Darwin could not hold his face close against its glass cage when a puff adder struck at him. Thus we can understand why some men who are heroes in their cortex cannot help being cowards in their pons or solar plexus. Revivals of old, long-submerged, dendritic or even pelagic stages of evolution thus may surge up and evict the judgment and master the will. These primeval reflexes may subject the most intelligent cortex, even although they are innate reflexes formed in primeval times. They thus have their innings. From this viewpoint no less light is shed upon the normal genetic psychology of shock and fear than it has lately brought to the analysis and therapy of neuroses and psychoses, and these two must not be separated from the emotions, as they have unfortunately been in the studies of the intellect and will.

Shock even more than normal pain brings a new fear, viz., that of its reflex effects. We dread self-betrayal to others and also the strain, pain and danger it brings to our own organism. To be liable to be reduced to an automaton and to play off these old reactions that it is the business of culture to repress, to unleash and exhibit these dragons of the prime that slumber in us, to be thus suddenly dragged back to the lowest meristic, if not almost vegetative, levels, brings its own special types of obsession. We grow tense, hold onto and repress other instinctive tendencies than those fraught with danger and so there is often a diffusion and substitution of restraints, usually sub-conscious, which it is the business of psychoanalysis to lay bare. These dormant forces in our nature, always on the alert to surprise us if occasion arises, seem uncanny and often supernal for there is nothing man knows so little of as of the lower strata of his own psychophysic nature and in no situation is he so prone to eject, personify, and deem himself possessed by supernal agencies or beings as when he is the victim of these recrudescences. It seems not his ego but the eruption of an alien being. It is thus in experiences like these that the supernatural has its last stronghold, as modern spiritism and telepathy show; and Stekel has pointed out in great detail how what man really most fears is himself, because his inner primal nature is that which he knows least and which may seize and control most completely both his body and soul.

To the geneticist the individual is a bundle of organs and traits of very different ages and stages of development as well as of usefulness. A few score of rudimentary organs, according to Wiedersheim, survive in the adult. Many reached their acme, declined and perhaps vanished before birth, while some organs and tissues seem growing, if not young, in old age. Some of the superseded ones are metamorphosed over into other organs useful to the adult. Some are mere vestiges of parts once of vital importance in some earlier phyletic stage. Possibly some are buds and they have their day again in some higher stage of life. True rudimentary organs tend to persist and they may become detrimental, absorbing vitality and becoming inviting seats of pathogenic organisms. Retrogressive organs are susceptible to lesions which may entail serious consequences for the individual. Thus some of them are old when we are young and perhaps our true physiological age is that of the average age of the different parts and traits, the long and the short lived taken together. Hence the infant is long phyletic stages older than the adult, and what we now call maturity, and still more old age, is a recent acquisition of the race resting upon far older foundations. Possibly all the Mendelian, Weismannian or Semonian and other hypothetical units that are supposed to carry hereditary impulsions and with the power to vary independently of each other, belong themselves to different stages of phylogenetic development, and, on the other hand in men, many differences in physical and psychic traits may be due to the different net age of their aliquot parts and the points of their arrest under influences which are always inhibiting or stimulating the all-dominant momentum of growth. In some individuals the average growth is so retarded that they retain juvenile and perhaps puerile qualities through life and seem never to grow old, as in ateleosis, while others hasten prematurely to their goal in porgeria. More commonly, however, both the acceleration and retardation are not general but specific.

From the studies begun by Roux, O. Hertwig, Born and Schaper more than a decade ago on amphibia and other lower vertebrates, and advanced by many others since, it seems now established that up to a certain point of embryonic growth, varying with species, each organ differentiates and grows by itself independently of others and quite uninfluenced by the central nervous system. They may of course compete with each other for the available nutritive material, but the morphological development of each is by and for itself and their association is only that of a mosaic. At first the cerebrospinal system neither receives nor sends fibers to the parts and it may be removed without affecting their course of development. The same has repeatedly been observed in anencephalous amyolitic monsters that may live up to or even after birth, while each part develops from its own hereditary impulse with little reference to the whole. At a later stage the central nervous system may exercise over the growing parts some trophic influence, but no functional stimulus from it is needed or received, and the organ grows without functioning and without correlation with others. At birth, however, functioning begins to take precedence and growth comes to be dependent upon it; *i. e.*, there was relatively very little before but now it is essential for the further stages of development. The nervous system now begins its great work of correlating and unifying the functions of the parts and organs and also psychic life, which is closely bound up with these processes, has here the chief of all its impulsions. At this epoch the components of the individual which have unfolded in comparative isolation are knit together into a more or less compact and harmonious organization and also more subjected to the rule of the nervous system and an era of synthesis and at the same time subordination is inaugurated.

Now begins the great work of compensation which A. Adler[1] has best characterized and which is the most important key not only for abnormal, as he deems it but, as we believe, for normal genetic psychology, a view which may be characterized as follows: Every subnormal (*minderwertige*) is more plastic and adaptable than normal organs or functions. Under the stimulus and protection of the central nervous system when it has taken the helm they may become not only more variable in other ways but may even become supernormal. What is more important, they may be compensated by other organs or functions with which they are correlated. Moreover superstructures are built which vicariate for them, supplementing their deficiencies. Thus recalling ,as we saw above, that man is a congeries of many organs in various stages of evolution and decline, the nervous system when it comes to power establishes a set of interrelations between those that are essential under the impulse of the will to live. Leaving some to decline and powerfully stimulating others to unfold and develop, by keeping them sufficiently but not too much in exercise, it reinforces both atrophy and hypertrophy. In the effort of the psyche to foster the important organs and functions which it selects for its special care, organic defect may be compensated by excess of nervous activity. Indeed, most compensations are in the psychic though not necessarily in the conscious field. No one is perfect, and hence compensation is necessary for all. It makes for, if indeed it does not make, consciousness itself. Those organs and functions which the psyche cannot directly or indirectly control decay or become stigmata. Where the brain fails to establish a compensatory system we have all the hosts of neuroses and psychoses. The existence of sub- or abnormal organs or functions always brings Janet's sense of incompleteness or insufficiency, and this arouses a countervailing impulsion to be complete and efficient which those to whom nature gave lives of balanced harmony do not feel. The ideal goal is always to be a whole man or woman in mind and body, and this may crop out in the childish wishes that are sometimes fulfilled in dreams, in the ambition of the boy who aches to be a man, and in general in the desire to overcome all defects and to evolve a full-rounded, mature, powerful and well-balanced personality. To illustrate, each bilateral organ compensates for defect in the other, one sense for another like touch for sight in the blind. Mozart had an imperfectly developed ear; Beethoven had otosclerosis; Demosthenes stammered and, as if mythology had recognized this law, many of the ancient gods were defective. Odin had but one eye; Tyr, one hand; Vulcan was lame; Vidar dumb. So, too, the ugly Socrates made himself a beautiful soul. A man with a weak digestion becomes a dietetic expert in battling with fate. Little men walk straight; tall men stoop. Handsome men are superficial. A subnormal eye intensifies the visual psyche. In the effort to control enuresis due to renal insufficiency over compensation may predispose to even dreams of water. Sex weakness is supplemented by fancies of superpotence. Many diseases have compensating forms with which they alternate or for which they vicariate and the very principle of immunization is involved. Weak parts and functions draw attention and are invigorated thereby. Fear of an object excites interest in it and this brings the knowledge which casts out fear. Very much of the total energy of all of us and still more of that of neurotics and psychotics is spent in developing and using devices of concealment (*Deckphenomene*) of diseases and defects. Thus often the higher protective and defensive mechanisms come to do the work of the subnormal function even better than it would do it. Conversely compensation has its limits and when it breaks down we have anxiety, the most comprehensive of all fears and the alpha and omega of psychiatry, the degree of which is inversely as the ability to realize the life-wish of self-maximization. It involves a sense of inferiority, inadequacy and great inner tension. The goal may be the humble one of self-support, normality, merely absence of actual pain, or deformity, but the prospect of failure to attain it brings a distress probably equalled by no other form of suffering and every fear is a special form or degree of it. If the good, strong, healthy, higher components can neither improve nor atone for the bad, weak, low or morbid elements, anxiety, conscious or unconscious, supervenes, values lose their worth, we tend to take refuge from reality in fancies, and innate momenta are arrested and we suffer we know not what, perhaps fear itself.

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[1] A. Adler. *Studie über Minderwertigkeit von Organen*, 1907, pp. 92, also *Über den Nervösen Charakter*, 1912, pp. 195.

Freud is wrong in interpreting this most generic form of fear as rooted in sex, worries concerning which are only one of the more specific, if common and most typical, forms of its expression. Sex anxieties are themselves only symbols of this deeper sense of abatement of the will to live, to be powerful, to illustrate in our own personality the whole estate of man, to glow with the humanistic totalising motive to be citizens of all times and spectators of all events. Of this sex anxieties are only parables, *modi dicendi*, or most ostensive instances of a larger general law. In the super- or sub-conscious structure of the soul, minus are cancelling plus qualities, and instead of hearty abandon to life there is halting, vacillation and uncertainty, if not incipient renunciation, which may go on to despair or utter loss of hope. Stekel has urged that the ultimate fear is that of death or annihilation, and infers that belief in immortality gives bravery when we confront the ills of this world but makes us cowards towards those of the next. This and the dread of death-dispensing agencies like disease have motivated all hygienic devices. Special phobias too often relieve general anxiety by concentrating it in one direction, as *e. g.*, acute fears of darkness may relieve us of anxieties during the day. When life is beclouded by pain love of it abates, although we do not know it, and sorrow makes us even less cautious against accident, so that, other things being equal, a man who is frankly happy is a better insurance risk, and as the biographies of Segantini and others show, has more power to resist disease. Sex fear plays a great rôle in psychopathology because its pleasures are most intense and all creatures are most vital when most procreant and because this function conserves the immortality of the race.

In a large and pregnant sense consciousness itself is compensation, and is the psychic aspect of a deeper biologic law. In geniuses as in neurotics, it comes more to the surface. Berger's story of a born criminal who became a judge and was noted for his Draconian severity but who lapsed to crime and committed suicide, leaving a confessional autobiography, is typical of one aspect of it. The work of great artists is often a complement of their lives, expressing in most ideal form what they most lack. If the heart, digestive processes, lungs, muscles, are weak or go wrong, they come into consciousness, and curative agencies are initiated. Pain is a cry of the lower, older parts and functions of our organism to the higher nervous system for help. Paranoiacs tending to delusions of greatness and hyper-self-feeling are often overpolite to others. The sense of defect prompts training and education to cure and also countless devices to hide them. Culture corrects the errors of instinct and dress hides deformities. Thus nurture supplements nature, and environment has to rectify heredity. These processes constitute consciousness, which is always more or less remedial. Taine conceived it as a mutual repression of opposite impulses and tendencies, any of which if not checked would develop into insane intensity, and he deemed the neuroses as only the most intense form of it. Where these integrating and compensating processes have more than they can do and break down, whether from strain of outer circumstances or because they find inner resistances too great, so that the power to rectify and adjust is exhausted, abatement of the life impulse is felt, and this sense of abatement is anxiety, diffuse or acute. It is the bi-polar opposite of the pleroma of life abounding, which all crave. From this point of view, then, consciousness itself is incipient anxiety.

This brings us to one of the most interesting and important questions of genetic psychology, viz., whether, and if so how, fear is inherited, to which the general answer may be given that the anxiety diathesis is one of the most inheritable of traits, while fears of special objects are so only to a very slight degree. Organs and functions little used tend to regress and become more rudimentary and embryonic. Compensation may shelter and relieve them by providing for their work vicariously and thus facilitate this process of decay. This, however, commits the higher vicariating organs and functions to more than their normal work, but this compensating hyperfunction is an acquired quality and so less hereditary and must be re-acquired in each successive generation, the regression of the primary older traits being much slower. Overworked organs and functions tend also to decline in the offspring, and so the disproportion becomes cumulative. Moreover, where components fall back they also fall apart, and the integration on which the wholeness of the ego depends is impaired, and normal synthesis impossible, even though the environment (every change of which requires new compensations and adjustments which may either help or mar) be most favorable. Now sex is the function by which these inner disharmonies are transmitted. Hence it comes that the *vis reparatrix naturae* makes here its greatest restorative effort. Love is the chief complementary agency, with its basis deep in the processes of cross-fertilization. Love is between not replicas but counterparts, and its aim is to supplement deviations from the norm, the degree of divergence of which seems to correspond to the passionateness of love. We love in others those traits we lack, and thus tend to correct our defects, and hence it is that neurotics turn to the love cure with most abandon. A deep, complex racial instinct, many factors of which are far below consciousness, impels to interest in the *vita sexualis,* which is the organ of the relations of the individual to the other sex and through it to posterity. From the above we see how prone its components are to be characterized by excess or defect, and thus prone to disorganization and most in need of compensation, while impairment, inversion, perversion and excesses are always found with polymorphic fears and manifold gross crassifications, sublimations, exaggerations and repressions, with their alternating extremes of abandon and control. Proclivity to these disorganizations of this fundamental and complex instinct, expressing the race in the individual, not only profoundly affects heredity by impairing its efficiency but is itself one of the most inheritable of all complex traits, so that disturbance here constitutes the rankest soil not only for anxiety but for specific fears. Such a diathesis predisposes to fearsomeness in domains quite outside its own and, as Freudians have shown, is easily transferred, takes on many substitute and symbolic forms, which the sufferer himself finds it hard to unmask and recognize for what they really are, so intense and incessant is the action of repression. But while this abnormal hyperconsciousness of sex may have recourse to almost any symbolic expression, we must not forget that for every even tolerably normal individual fears of losing power, possession, or of accident, animals, diseases, outbreaks of anger, etc., have their own independent causation and use not only many of the mechanisms which recent sex studies have revealed in it, but others that have no connection with sex, so that predispositions to these affectivities are peculiarly prone to be inherited. The *summum genus* of fear thus is a sense of the inability to cope with life, a dread of being vanquished and becoming not victors in its battle, a sense of limitation and of inferiority in our power to achieve the fullest success and happiness, a feeling that our hereditary momentum was originally insufficient or is in danger of being reduced. We would do, be, get all that is possible for man's estate, attain the fullest macrobiotic development, and fear and shock are intimations that we fall short, are less than we might, could or should be. This excelsior impulsion encounters obstacles and suffers arrest, and desire, ambitions, possibilities, may fail. Hence pain and its anticipation, fear, and their diaphrenic opposites, pleasure and hope, play a great rôle in the evolution of the affective life, not without analogies to that assigned to nothing and being in the Hegelian logic. The thesis, antithesis and synthesis of the one are the basis of an affective, and those of the other of a rational, dialectic system. Hope and fear have had very much to do in shaping not only habits, instincts and probably structure itself, but in making mental and nervous disease or health. Indeed from the genetic standpoint they are the creators of consciousness itself, from its lowest to its highest form.

For years I have kept tab on the phobias or morbid fears described in literature, chiefly medical, and these are tabulated below. Some have an extensive literature and some only brief mention, illustrated, perhaps, by a single case. Besides these there are very many others, mentioned in the description of cases, for which no Greek name has been suggested; for instance, fear of the several hundred diseases which the English Pharmacological Society attempted to catalogue, many of which have been objects of abnormal fears. Many, if not indeed most, objects of nature have been feared unreasonably at least for a time, especially by children, who normally have and outgrow many phobias. About every thing and event at first may excite undue fear, which experience tends to eliminate or tone down to reasonable intensity. No good classification of phobias exists, but they will doubtless be reduced to something like genera, species and varieties, though no psychological classification can ever be based upon objects, for these are largely accidental and generic fearsomeness or anxiety readily changes its objects.

MORBID FEARS

Phobia	*Meaning*	*Phobia*	*Meaning*
Acero-	Sour	Demono-	Demons
Achluo-	Darkness	Dermato-	Skin
Acro-	Sharpness	Dike-	Injustice
Aero-	Air	Dora-	Fur
Agora-	Open spaces	Eisoptro-	Mirror
Aichuro-	Points	Elektro-	Electricity
Ailouro-	Cats	Entomo-	Insects
Akoustico-	Sound	Erete-	Pins
Algo-	Pain	Eoso-	Dawn
Amaka-	Carriages	Eremia-	Solitude
Amatho-	Dust	Ereutho-	Blush
Anemo-	Wind	Ergo-	Work
Angino-	Narrowness	Geno-	Sex
Anthropo-	Man	Geuma-	Taste
Antlo-	Flood	Grapho-	Writing
Apeiro-	Infinity	Gymnoto-	Nudity
Arachne-	Spiders	Gyne-	Women
Astheno-	Weakness	Hapto-	Touch
Astra-	Astral	Hamarto-	Sin
Ate-	Ruin	Harpaxo-	Robbers
Aulo-	Flute	Hedono-	Pleasure
Aurora-	Northern lights	Hemato-	Blood
Bacillo-	Microbes	Hodo-	Travel
Baro-	Gravity	Homichlo-	Fog
Baso-	Walking	Horme-	Shock
Batracho-	Reptiles	Hydro-	Water
Blone-	Needles	Hypegia-	Responsibility
Bronte-	Thunder	Hypno-	Sleep
Cheima-	Cold	Ideo-	Ideas
Chiono-	Snow	Kakorraphia-	Failure
Chrono-	Duration	Katagalo-	Ridicule
Claustra-	Closed spaces	Keno-	Void
Cometo-	Comets	Kineso-	Motion
Cremato-	Money	Klepto-	Steal
Cromo-	Color	Kopo-	Fatigue
Chrystallo-	Crystals	Kristallo-	Ice
Cyno-	Dogs	Lalio-	Stuttering
Deme-	Crowds	Linono-	String

Phobia	*Meaning*	*Phobia*	*Meaning*
Logo-	Words	Phasmo-	Ghosts
Lysso-	Insanity	Phobo-	Fears
Mania-	Insanity	Photo-	Light
Mastigo-	Flogging	Pnigero-	Smothering
Mechano-	Machinery	Poine-	Punishment
Metallo-	Metals	Poly-	Many things
Meteoro-	Meteors	Poto-	Drink
Miso-	Contamination	Pterono-	Feathers
Mono-	One thing	Pyro-	Fire
Muso-	Mice	Rypo-	Soiling
Musico-	Music	Satano-	Satan
Nekro-	Corpses	Sela-	Flash
Nelo-	Glass	Sidero-	Stars
Neo-	New	Sito-	Food
Nephelo-	Clouds	Sperma-	
Noso-	Symptoms	(Spermato-)	Germs
Ocho-	Vehicles	Staso-	Standing
Odonto-	Teeth	Stygio-	
Oiko-	Home	(Hade-)	Hell
Olfacto-	Smell	Syphilo-	Syphilis
Omata-	Eyes	Thaaso-	Sitting
Oneiro-	Dreams	Thalasso-	Sea
Ophidio-	Snakes	Thanato-	Death
Ornitho-	Birds	Theo-	God
Ourano-	Heaven	Thermo-	Heat
Pan- (Panto-)	Everything	Toxo-	Poison
Partheno-	Girls	Tremo-	Trembling
Patho-	Disease	Zeino-	Strangers
Patroio-	Heredity	Zelo-	Jealousy
Penia-	Poverty	Zoö-	Animals

We pass now to a few of the most generic types of fear. (I) *Fear of shock*. Experience as it unrolls moment by moment may be roughly divided into two kinds, the ordinary and extraordinary, or the expected and the unexpected. To the former we are adjusted by habit and the sequence of events in it is anticipated, and we respond sanely and naturally. The other class of events is sprung on us without warning, such as accidents, death, perhaps sudden loss of property or good name, involving rupture of continuity of the stream of consciousness. The simpler forms of shock are very common in the life of children, of which we have many hundred records before us of many kinds, from being said "Boo" to or "jumped out at," hearing a new clock strike, a Jack-in-the-Box, being stolen up to or touched unawares, a discharge of firearms or any other sharp noise, barking or attack by a dog, a crash of thunder, a sudden sneeze, etc., to being run away with, burglars, shipwreck, cyclone, earthquake, falling into or having cold water dashed over the body, suddenly finding some one present instead of being alone, as was supposed, a shock from a battery, a blast that went off near, fires, accidents, or bad news of near friends, many of these effects being greatly increased by darkness. Many children's plays focus in causing a start, a sudden surprise or alarm. Probably both physical and psychic shock is rather more common with children because of their lack of experience than with adults, and the element of shock is not infrequent in their dreams. For normal children an element of shock in their sports seems to have a veritable charm, and they appear to be strengthened and satisfied by learning to suppress its ill effects and in securing the optimal degree of it that is most favorable for the development of self-control and presence of mind. This differs very greatly with individuals and is often dangerous to neurotic children. The very incoherence of the mental processes of children makes shock less perilously disruptive than it is in later life after careful observation, associative thinking, together with wont and custom, have knit the factors which compose our mental life into a more complete unity. The reactions of children to shock, as we should expect, are very manifold. Some grow rigid, with every muscle tense; others limp, paralyzed, and perhaps almost ready to collapse. Creepy-crawly horripilation symptoms are often mentioned. The heart throbs, there is flushing or pallor, the eyes are fixed, the mouth opened, they pant, perhaps make a wild rush, scream. Both facial expression and attitude are very similar to those found in exophthalmic goitre (Grave's disease), or in the photographed faces of athletes at the crisis of their efforts. In shock the mind fails to grasp the whole situation and so the active responses are ill-adapted to meet it. There is often a period of utter mental confusion although sometimes only an initial start followed by an outburst of rage against the cause of the fright, and manifestations of mortification at the betrayal of weakness and loss of poise, although others can laugh it all off as a joke. On the other hand, shock is often the nucleus of a phobia which irradiates by association to many items of place, person, and circumstance, and which may persist for years. Psychic, cardiac, neural and many other traumata may thus be caused either suddenly or with onset so gradual that the symptoms do not suggest the real cause. One child, *e. g.*, saw a case of sunstroke and had for years almost a mania for keeping in shady places and would go far around to avoid sunshine. Another child saw a barn blown down and became an anemophobiac, and so we have phobias of animals, lightning, cars, burglars, fire, firearms, and some of these long dreads evolve a set of fantastic precautions sometimes endless in both variety and detail. Most children have many of these fears in intense forms, perhaps in silence and unbeknown to their friends, and completely outgrow them later. Sudden phenomena thus have a psychology of their own.

If pressure be applied gradually enough an excised motor nerve may be crushed without causing the muscle to which it leads to contract. It may also be heated, cooled, electrified, acidified or alkalined to the lethal point so gradually as to cause no movement. Normal frogs may be placed in water heated so slowly that they may be boiled to death and make no movements. So pressure may be applied to the human finger so gradually as to pass much beyond the ordinary pain point without any perception of pain, and movement on the skin may be so slow as not to be perceived. Certain secular cosmic changes follow the same law. Thus we may conclude that to excite consciousness every change must have some degree of suddenness, and Spencer finds the very origin of mind in some primitive form of shock. Below certain limits of abruptness in change of stimuli to which it is a response, psychic life cannot exist. Perhaps anything might occur to us without our knowing it, if it came slowly enough, while, conversely, the most common gradual changes if they came with great and unusual suddenness would cause shock. Thus here again we have a time function. Again, subjectively there is some doubt whether we can perceive gradual changes as such. Instead of an image or feeling of continuous increment or decrement, some believe that we derive it or infer it from a comparison of graded stages, which the mind seizes upon in the series, and that the intensive continuity is like that of the cinematograph. Others hold that the experiences of continuity are primary, as notes are measures of changes of ungraded pitch like those of the Aeolian harp, which preceded and conditioned the scale, for their intensity changes are primordial, as Trendelenburg made motion his prime category. For them degrees and gradations are later applied to changes which were originally without them, a view on the whole more consonant with geneticism as well as with introspection.

Between their threshold and the degree of intensity which causes shock, stimuli vary greatly in strength and evoke their normal responses with much regularity, depending upon fatigue, temperament, experience, preparedness, etc., and it is within these limits that by far the most of what we call experience takes place. Below this threshold lies a vast field of unconscious response to faint and slowly changing stimuli.

Training, interest, and many other influences are constantly widening the ever-variable interval between just-perceivable alterations and their upper limit where shock begins. Between these limits increase of both intensity and suddenness of stimuli is met by increased intensity and lessened reaction time in the response. Shock, however, always brings perturbation, cerebral commotion, confusion, and many pathological symptoms, both psychic and somatic. One group of theories represented by Goltz, Gross, Schneider, Crile, Blum, Agnew, Brewer, think that the cause-effect of shock is chiefly circulatory, either loss of vaso innervation involving the vagus or pneumo-gastric reflex, with increased or lessened blood pressure or changes in the constitution of the blood itself, both of which undoubtedly occur. Others, Groeningen, Oppenheim, Hodge, Warren, Strumpel, find the lesion chiefly in the nervous system. The nucleus of its cells shrinks and becomes vacuolated, darkens by becoming charged with the uneliminated products of decomposition due to excessive katabolism, and some place the center of exhaustion in the medulla or spinal cord where spots of degeneration may appear. Henderson thinks general or local apnoea or lack of oxygen is the chief effect, at least in cases of surgical shock. Another theory suggested by Erichsen in 1868, and revived and much discussed a decade ago by neurologists, is that the processes that connect one nerve unit with another make ameboid-like movements, bringing them into contact with others, which represents association, or breaking these connections by retraction, as in cases of shock, and thereby causing psychic dissociation. Amoeba tend to ball up if disturbed, retracting their processes and presenting less surface, and on this theory the nerve elements retain some slight degree of this power. In this way certain psychophysic groups of elements are more or less excluded from the influence of others and act independently because the whole has lost control over the factors that compose it. Thus even dual, multiple or parasitic personalities or complexes are split off or isolated from their connections. Shock thus literally shatters the psyche through its neural centers and tends to resolve it into its proximate elements. If any such movement of terminal cell processes is possible, we might well expect that shock, which is the most drastic of all experiences, would be most likely to cause it. It might indeed have much to do in forming the plastic brain of the child and especially in isolating functions that education and experience seek to integrate so that the individual shall act as a whole. This theory thus proposes a physical basis, not only for shock and schizophrenia, but for fatigue, sleep, dreams, imperative ideas, etc. Some think that on this hypothesis shock may even in a secondary way cause new associations, the creation and rupture of which are due to connection and disconnection of the processes between more or less independent neurones. This kind of movement, however, is not at present satisfactorily proven, although Harrison thinks that traces of it appear in the growing nerve fibers of the embryo, but these like the movements of contact-granules at the distal end of dendritic processes may be significant only for metabolism or perhaps growth. Again, as the neurone theory has of late more or less made way for that of the synapse, shock has often come to be considered as a sudden lowering of the resistance, or an increase of the permeability of the synapses involved. On this theory shock is not due to irritation from the brain or inhibition, but something like a rupture of tenuous septa of aboral conducting paths. Neuropsychic connections depend then not on the make and break of nerve processes but on the permeability of synapses, which ranges through a long scale of degrees dependent upon habit, experience, fatigue and even heredity. When the synapse is flushed in shock as by a freshet, these tiny resistance surfaces are made more permeable and some of them that would have withstood almost any normal process may almost break down, and then these impulses irradiate into channels that should have been, and perhaps have been in the history of the race and the individual, long closed to them. Shock always tends to discharge downward in the more primitive zones, instead of in the forms that modern civilization has made usual, as if the synapses that should bar the way to these old types of reaction were too weak to do so in psychopathic constitutions.

Shock may affect about every sense and function. In the field of sight it may disturb accommodation, modify the size of the pupil, the tone of the retina, the eye-ball may roll or be fixed with almost cataleptic rigidity, there may be photopsia or subjective sensations and even color-blindness. In the field of audition it may cause deafness, transient or even permanent, and may bring supernormal acuity of hearing, as well as cause vestibular vertigo. In the domain of the dermal senses it may cause anaesthesia, local or diffused, and its effects may extend to the corresponding part of the body on the other side. Subjective skin sensations often appear and while the sense of contact is unimpaired, that of pain and temperature may be affected, one or both. In some cases the trauma affects smell and taste. Stammering and other modifications of speech and of the quality of voice sometimes ensue. Very many are the effects of shock upon motor control. There may be tremor, rigidity, flabbiness, paralysis, and various automatic and uncoördinated movements. Manifold changes occur also along the entire alimentary tract and its annexes. In place of appetite we have nausea and there are various excremental irregularities. The digestive process sometimes seems to come to a standstill while circulatory and respiratory activities increase. Neurasthenia and even insanity occasionally result, involving many neuroses and psychoses. All this is abundantly illustrated in the voluminous clinical literature upon the subject. Geneticism assumes that every one of these and all other responses to shock are vestiges of once useful reactions, that the visual symptoms are relics of an effort to find and then fixate some dreaded object, that accommodation would be, on the principle of self-preservation, first for a near point and then for one farther off, and that when the object was found the gaze would be fixed upon it with an almost cramping intensity. Studies of the stages of awakening from dreams show that mental may produce optical images by eccentric projection, and all the retinal, as well as the changes of eye-muscles, resulting from shock, would probably be caused by an intense effort of all the central and peripheral organs of vision to get the source of danger into the focus of the field of vision. In such drastic impulses the power to find and the power to make an object seem to shade into each other, perhaps by insensible gradations. When we are shocked there is an optical *Einstellung* that an object is to be seen, and this is so sudden and intense that if the cause is not seen, subjective sensations may sometimes take its place. As to the ear, it is known that unexpected detonations are more likely to injure the organ than are those that are anticipated, as well as that most shocks of early infancy are in the field of this sense. Next to the eye the ear is the distance-septor that guides movement in crises, while the function of the semi-circular canals would be strongly appealed to by way of giving equilibrium for either fight or flight. Auditory sensations too are great muscle tonors, and introspection shows that many people hear music in their muscles, so closely wed are sound and movement. While blindness and deafness would be lesions helpful only to foes, and hyperfunction of these senses would be normal, the dermal anaesthesias that are common in shock would be very helpful in a life and death struggle with enemies, and also with the forces of nature, as this would render the contestant unconscious of, and so undisturbed by, his injuries. A conflict, too, would involve rise of temperature, activity of not only the sweat but salivary, and perhaps other glands, although the functions of some seem to be inhibited. If one side of the body was disabled the other would naturally be called into function. In mortal combat the very odor of man's animal foes, and, when he once used his teeth, their taste, would be a painful part of the experience. Speech, if it had been acquired, would be useless, and would be superseded by inarticulate and reiterated cries for help or to terrify the foe, while the timbre of the voice would take on a new quality. Tremor due to shock is a relic of what was perhaps once readiness of muscles and limbs to act in one of perhaps several opposite ways and we quake because our forbears used to be for a critical moment equally ready to do a number of the most diverse things, the original fluctuations being those of indecision, while in the motor incoördination that is so characteristic a symptom of shock, we see the residua of brief selective struggles between older or phyletic and newer types of response. To eat immediately upon entering on a life and death struggle would be fatal if the contestants were otherwise equal, hence now we feel not only no appetite but nausea when on the ragged edge of acute danger. Excrementation lightened the body and made it more agile. The more finished products of the later stages of digestion, on the other hand, would be chiefly and immediately serviceable, so that heart and lung action and also blood pressure would be augmented to their maximum, perhaps to the point of lesion, while the primary grosser digestive activities, which would divert energy, were suspended. The most urgent call for the most instant activity would, however, come to the nervous system, for to it falls the greatest and most important task of selecting, coördinating and innervating the most advantageous movements best adjusted to the clearest perception of all the pertinent factors of the situation. How inadequate this, its work, is in cases of shock we see in many of the phenomena above wherein obsolete motor patterns are more or less activated, harmful though they now are. Especially in neurotics these may come to absorb most of the energy needed for rational responses, and synapses that should be impervious are broken through. Hence shock has strange power to initiate the most diverse neuroses and psychoses, all of which if they could be adequately analyzed psychogenetically, would be found to be reversions to, and also perhaps more often than we suspect, actual magnifications of acts and psychic states that were at one time the fittest of which our forbears were capable. But again we see here as so often elsewhere that it would be unjust to primitive man and his ancestors to assume that all such pathological phenomena of today are mere revivals of his states and acts. They are often, on the other hand, grotesque variants and intensifications of phylogenetic originals that were more sane and simple if also more generic. Shock symptoms may thus be symbols of long past racial experiences which when we have learned to interpret them more fully will tell us much of the early history of our phylum.

Now not only does every shock leave its own fear behind it but no fear is more universal in all forms and stages of life than that of shock. Many of the most remarkable instincts of animals, such as the posting of sentinels, wariness, forms of shelter, were developed to avoid the surprise of being caught unawares. Apprehensiveness has its own *Einstellung* or mental set which normally mitigates shock effects by expectant attention, that seems to be able to open and facilitate the ways to adaptive and increase the resistance to unfit modes of response. Although the *Einstellung* is itself more generic and indefinite than the shock it anticipates, it makes us ready to a degree which is the inverse of its generality for the more specific shock experience when it comes. In neurotics, however, this fore-feeling of shock may be as vivid and explicit as the shock itself, and particularly in hystericals, about one-fifth of the cases of which are traumatic in origin, it often seems to be even more so. How comes it then, that phlegmatic and lymphatic temperaments, hibernating animals and human foetuses, which may survive the amputation of all four limbs, endure so much graver surgical shock than can civilized man? Is it because their cerebral centers, being less highly developed, are themselves more immune to lesions, or because the strain of expectancy is reduced? If the chief factor in shock is of neuropsychic origin, it would seem that *Einstellung* would increase and not decrease its deleterious effects. Perhaps the explanation must be sought along the following line. Most if not all anaesthetics as well as narcotics and hypnotizations do not obliterate consciousness but retract it from present reality to an intensified dream-revery, from which even a severe operation does not awaken us. Under its influence, then, we are not mindless but only intensely preoccupied or distracted, so that the pain is unnoticed and so not reacted on. On the same principle wounds are not felt in the excitement of battle or, as many records show, when men are being torn by beasts of prey. The lower automatic responses may go on and the victim may groan, feel nausea, void excrement, flush and grow pale, circulation and respiration may be modified, much as if the pain had really entered the centers of normal consciousness, although perhaps even the voluntary muscles are paralyzed. Why, then, does not man tend to meet all sudden strong pains of life by the method of diversion from rather than that of the clearest envisagement? Why not put all thought of anticipated shocks and pains out of mind and perhaps deny their existence instead of striving to get them into the very fovea of apperception before they occur and even while they last? The former more often goes with the method of passive fatalistic resignation and quietism which accepts the inevitable, and the latter is that of revolt or at least intensive quest of ways of escape. By one way we tend to be masters and by the other victims although we must reverse the Freudian derivation, which makes this distinction originate in that between Sadism and Masochism instead of regarding the latter as secondary, as we must. One affirms and the other negates the will to live. Each has its psychology, its philosophy, and its religion. One accepts illness, death, and even annihilation without resistance, while the other passionately posits life here and even hereafter.

Humboldt says that the shock of earthquake is not at root the noises, human suffering, destruction of property, but the sudden reversal of the age-long sense that the solid earth is immovable. The victims of the "Titanic" accepted the prospect of death with composure till its fatal plunge into the cold water, which brought the automatic struggles and shrieks which were beyond control. Earthquakes, pestilences like the "Black Death," which between 1348 and 1350 slew one-fourth the population of Europe, brought unprecedented despair and abandonment of all moral, social and religious restraints, and even decency, set free about every bestial passion, lust, murder, debauchery, etc., yet there were those who, facing this most painful and disgusting form of death for themselves with equanimity, were not only faithful to the end in their ministrations, but were able to use their reason coldly in describing the malady and its effects and in striving to grasp its cause and cure. Much of the entire history of civilization, custom and religion, and indeed of progress generally might be written in terms of the decrement of the element of shock, although while we are learning to mitigate old we are causing many new forms by way of accidents. The conception of the world as a cosmos instead of chaos, of every event as a link in an endless chain of cause and effect, the growth of foresight, prevention, insurance, training to meet emergencies, has been the work of those who faced and disarmed shock and its horrid cortege of fears. This at least is one of the psychic causes of bravery and courage, the touchstone and reward of presence instead of absence of mind which gives power and self-control in emergencies.

It must therefore have a deep genetic root, and if so what is it? For answer we must recall the law of ambivalence. Along with their fear of it virile man and his ancestors always loved shock. He has been an adventurer, a plunger, courting the chance of it, and this has been one of the spurs of curiosity and enterprise and constitutes a part of the charm of novelty and its opposite—the tedium of wont and routine. By his quest of change and hazard in its many forms man both tempers and tests his mettle for each bane prompts him to find an antidote and each toxin an antitoxin. By this process man feels himself, summates his powers, focuses and unifies all the factors of his personality, abandoning for the nonce all motives of caution and prudence for the fascination of danger, alternating between them, and, in protracted periods of each, accumulating incentive for the other. Thus we have here the law of combination of attraction and repulsion, neither ever absent but each alternately predominating, much as appears in the antithesis between sex impulsion repressed by modesty, convention and morality, with its lure of the risqué.

Only geneticism can explain the above paradox. As life has evolved on the psychic plane, the preservation of life against all great and sudden injuries is less by the vegetative power of recuperation and more in the cerebral function of pre-perceiving and so preventing shock. Instead of the power of regenerating mutilated tissue or even lost limbs and sense organs by the *vis reparatrix* of re-growth, men, especially those well endowed with brain and mind, depend increasingly for survival upon their keenness of perception and their foreknowledge of coming harm, which enables them to escape it. Once continued existence depended upon the degree of injury which could be made good by re-growth, and in lower races and still more in animals we find almost incredible restorations. This power was of course of the very highest value in the conservation of the race, comparable with and perhaps even more marvelous than the achievements of modern antiseptic surgery. This old regenerative power is now less needed and so less developed in the recent racial experience of highly evolved man because he is more able to protect himself from grave injury by having learned to fear aright, *i. e.,* preventively. With evolution also goes the fact that the number and gravity of psychic more and more preponderate over that of physical shocks and traumata. Forcible coition too once involved violence and perhaps physiological injury, while now the moral and social damage comes to the fore. In view of all this no wonder that meddlesome men have a strange proclivity to exercise these new powers and to put them to the test, to let themselves out, to court danger, to owe their integrity of body to superior knowledge and not to mere restorative growth. Yet we must understand that the latter was a genetic fore-school for the former. If primitive man had not had a high degree of disvulnerability or recuperative power he would have been today less brave, if trivial accidents had killed he would hardly have survived at all, and what is more important, he would not have profited by the wisdom and caution which his daring danger and hardship brought him. Moreover primitive medicine, we are now learning, was not so ineffective for those who practised it as was once thought, and also, however little or great its value, the fact that its devotees had such faith in it made them brave. So, too, did every form and degree of a belief in a future life, in which courage is always rewarded. Thus as the operations and accidents that the cave man survived attest his vitality, as also do the unhygienic conditions which he survived, his faith in the arts of his medicine men and his certainty of another life were resources that bred a diathesis of courage which we inherit in our instinct to take large risks, gamble with fortune and dare the heaviest blows of fate. Our ventures are with the more impalpable dangers of loss of fortune, good name, social or moral calamity, and our trust is in the foresight that escapes and we think less perhaps of rehabilitation in case of defeat. " Nothing venture, nothing have " expresses a deep instinct that has thus a venerable pedigree and that is the best of all antidotes of fear.

Many mental and neural diseases are caused by shock of many kinds. Of over 6,000 cases of insanity it was lately estimated that about 5 per cent. were due to shock, while of hysteria Freudians think it the only cause. It may bring on amnesia, hysteria, and even amentia. It may cause abrupt changes of character as well as of health, and it is a fertile mother of phobias. Its injurious are more studied than its curative effects although the latter are undoubted. An insult that causes a sudden explosion of rage, threats and abrupt frights, flagellations, earthquakes, a pail of water dashed over a patient, a fire or other sudden danger, news of a death, injury or accident—all these have many surprising cures to their credit, discredited as this method of therapy now is despite its tonic effects. Any sudden and intense emotion may have great transforming power upon thought and life. The consciousness of being in love may come as a sudden shock, energizing and intensifying everything, and being almost as revolutionary as was the old instantaneous conversion; so may rage and hate, jealousy, etc. Habitual consciousness may be evicted by an eruption from the unconscious regions of the soul and a new personality, either better or worse, may come to the fore, for since Kerkengard we have striking conversions from a good to an evil life as well as conversely. All such phenomena the old theories of possession made objects of awe if not of fear. Reason always fears emotion and is shocked by its outbreaks, and well it may be, for they mark the incursions of the race into the narrow life of the individual. It is this dread that has evolved a vast array of regulative apparatus, from rules of etiquette, the morals of passional control, criminal law, and above all religion, which is man's supremest device for the regimentation of his feelings. When they break out riotously in the individual or in the mob they may in a moment wreak a havoc that nothing can make good. Hence it is our own emotional possibilities rather than the moral law, as Kant thought, before which we stand in supremest awe. Their sublimation, directly or indirectly, is almost the whole work of culture. In this sense the fear of self is the beginning of wisdom. Every supernatural object or personality is the creation of these feelings. All of them we fear but only secondarily for the fear in which they all root is that of self.[2]

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[2] One of the chief dangers of grave surgical operations is shock, due to the great exhaustion caused by the dread and fear which the patient feels for the operation in advance, and the traumatic effects due to the laceration of tissue. In other words there is first the anticipation and second the injury itself. These together pump out the stored up energies of the brain and bring extreme exhaustion, which often issues in death. Both these dangers have been greatly mitigated by the method of Dr. Crile, the enthusiastic advocates of which make it a discovery equal to that of anaesthetics and aseptic methods. To obviate the preliminary dread so that the patient shall not know when the operation is to take place, he is given on several days a slight anaesthetic which he is told is to facilitate preliminary examinations, but when the day arrives he is given, unbeknown to himself, a stronger dose usually of morphine and scopolamine to kill fear, which does this by its marvelous power of preventing asso-

Perhaps the most important component of the impulse to defy and quell fear is found in the domain of sex. Many forms of animal and human courtship involve valor, risk, prowess in conflict, and victory always charms, thrills and often wins the female. The hero who overcomes his foes more easily overcomes all her resistances and her defences weaken and grow less reliable in the contemplation of great deeds of vigorous and valorous males. Her sense of security in her own virtue is a trifle shaken in the presence of tokens of resistless masterfulness, and such manifestations ally themselves of course with, if they do not fan, her desires. The male who overcomes others could more easily overcome her, and the more aggressive the courtship the less her fault if she succumbs. Such a partner, too, can protect and provide for her during the helplessness of pregnancy and lactation. On the other hand through measureless ages the male has come to associate victory and lust, whether in the conflict of rivals for the boon of her favor, in the forays of exogamous tribes or in the orgies and rapes that follow the victories of armies, when all the accumulated energies of passion are let loose. Thus hardship and danger have been inveterately associated with sex indulgence. It is for the advantage of the

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ciation. A patient thus doped has not the slightest apprehension about what is to happen so that all danger of psychic shock has disappeared. The traumatic shock still remains and this is affected by a local anaesthetic such as cocaine or novacaine, which entirely destroys sensation in the parts to which they are applied by paralyzing the nerves so that they can transmit neither fear nor motion, so that radical and prolonged operations can be performed without deteriorating effects upon the brain. Thus the other terror of modern surgery, viz., the exhaustion that follows an operation, is greatly reduced if not almost eliminated. Under this anoci-association the number of deaths which in one large hospital under the old method was a little over six per thousand is reduced to less than one in a thousand. Crile's discovery is based on his phyletic theory that the brain developed its power of accumulating energy in the old war of all against all in the struggle for food and procreation. The brain was thus made a great storehouse of energy, which is as real as steam power. Noci-septors are like push-buttons placed in those parts of the body most liable to injury and thus to need an influx of energy from the brain. Tickling, for instance, is a form of shock which is very exhausting because spasms were once life preservers. As brain energy is exhausted and when the hemispheres become like dead batteries man dies. Emotions produce the same mechanical trouble in the brain as does physical injury, and in old times fear was the chief disturber. A man on an operating table was very like our jungle-prowling progenitors in the grip of some tearing, rending monster, so far as its effects on the brain are concerned, until anaesthetics.

species that acute jeopardy to the individual should ally itself with the racial instinct of propagation. The brave not only deserve but win the fair and thus their quality is transmitted to posterity. The young savage brave cannot marry until he has staked his life in some way. Thus threatened negation of the individual is compensated by affirmation of the species. Hence even Sadistic impulses are the sequel and not, as Freudians assert, the antecedent of aggressiveness against others. This, too, is the order of development in childhood where, as always, the general precedes the special. The highest daring, then the highest pleasure, for individuality thus best documents itself as worthy of fruition. The charm of such ancestral rewards is no doubt inherited as an impulsion in the prepubescent boy to do or dare, but it appears in him only as a blind instinct with no awareness of its goal and with no glimmer of a sense that to take shocks that imperil personal existence means that in nature the race is always true to itself though reckless of the individual and at the latter's expense.

Finally, modern psychiatry is coming to assign to shock the chief rôle in nearly all psychoses and neuroses. On this view an intense, sudden, painful experience, especially if the significance of it can be dimly felt but not understood, may persist long and latently unassimilated by the central consciousness and without fusion with it, almost as if it were a foreign body in the psychic system. Such an experience may become the nucleus of a complex which without being recognized may grow into a dominant factor in the victim's life and become a parasitic or secondary personality. When it manifests itself in acts, ideas, dreams, wishes or otherwise it is usually so transformed that only analysis can interpret its real meaning. Not only the original fission itself but all the modifications that follow are products of fear, the first of shock, the last the dread of an internal censor. Freudians hold that most such experiences lie in the sphere of sex. A child who sees a sex act or anything else which shame usually conceals; a girl whose trusted and respected lover suddenly makes indecent advances; even a two- or three-year-old boy in whose presence a maid servant partially exposed her person; those who have experienced sudden acts of sex aggression; even the sudden but belated knowledge of how babies come to exist, if it comes in a coarse way;—these illustrate Freudian shocks as they manifest themselves at once or long afterwards by obsessions, complexes, motor, digestive or other symptoms and which may burrow into the psychophysic region of the organism, affecting conduct, mentation, dreams, etc., because such experiences cannot at once be faced and digested by consciousness, so that reaction to them is excessive or perverse. In such cases normal conscious reactions are supplemented and perhaps supplanted by those that are unconscious, instinctive or spontaneous. The first knowledge of the origin of human life may cause a shock. So may later first menstruation, spontaneous emissions, with sudden experience of the extremes of tension and relaxation, which is often extreme near the dawn of puberty. Then there is parturition, the instant shattering of love's idol, even the instant of awakening to the consciousness of a love which has been silently growing strong without awareness, the strain and excitement of being married, the revelations of the honeymoon, the care of establishing a new home, perhaps the sudden realization of the falsity of one party by the other:—these common experiences show that the *via vitæ sexualis* is a rough road to travel with many jolts in it, each of which has its host of victims. Thus it is no wonder that those who achieve the stormy voyage from adolescence to senescence sometimes feel, as the psychology of old age shows, that they have glided out of tempestuous seas into landlocked, calm, still waters, and that the philosophic quietude which writers from Aristotle and Cicero down thought a fit characteristic of this stage of life, is within their reach. Fears always troop about love and die with its death. Even more than in childhood individuality can affirm itself and is emancipated from the vaster interests of the race. The shock of conflict between the two abates. So long as the ego is freighted with the greater burden of the species there is incessant internecine war between the two, victory alternating from one side to the other, now one, now the other suffering defeat, physical, mental, moral, and our asylums, courts, and far more lives outside them, show the wreckage and debris of this struggle. Hence the theory that finds in shocks in this domain of life the chief cause of psychic disintegration and seeks to trace the connection between specific causes, and their effects merits today the greatest attention and respect, and every danger here has its own unique fear. (See more detailed treatment of this theme in the chapter on Sex-Love.)

In the past shock has perhaps done its most transforming work in this sphere of religion. From the Shamans, witch-doctors, medicine men, Dionysiac and Eleusinian mysteries down to recent revivalistic methods of conversion, shock has been an essential if not central agent of psychic metamorphosis. Apparitions of the dead, blood-curdling ghosts, dread Maskim, furies, gods and demi-gods, startling miracles that broke the order of nature, rapt, ecstatic, visionary states, trances and seizures, that broke the continuity of psychic life typified by often very elaborate symbols of death and re-birth:—all these represent supposed eruptions of an alien supernal power, suspending or breaking the sequence of natural law. Such things have always been the chief recourse of priestcraft and have often marked the submergence of the old and the rise of a new personality. Revolution took the place of evolution and saltatory has gone along with gradual development. In all this fear has been the chief motive appealed to. The sacrifice offered by worshippers means "*do ut abias,*" "I bring oblations to ye awful and unknown powers that ye may go away." So strong is our hereditary proclivity to experience religion spasmodically that it will be very long before the great transition from a life of selfishness and egoism to that of service and altruism will cease to be attended by convulsive symptoms because the idea of conquest by an alien power seems easier to grasp than that of normal growth, and perhaps hell and devils and all their awful kin will never cease to be horrid fear fetiches.

Thus we see that shock-fear has played an immense rôle at all stages in the development of animal and human life. It has given to creatures preyed upon the power to summate all their available energies in quick modes of escape and this power their enemies had to emulate. It has given man his power of second breath and general erethism and thus equipped him for meeting great emergencies. It has given him whatever spurty diathesis he has and made his progress upward not always that of the turtle but of the grasshopper. Even summital moments when epoch-making decisions are made or when inspiration comes or great thoughts and deeds evolve, all this is because fate has dealt out experience to man and his ancestors in a lumpy, bunchy way, to which his organism has grown wonted to respond. Hence it is that if his environment is too long monotonous, periods of excitement in which he breaks out now arise by an inner because they have had to so inveterately arise by an outer necessity. Thus we may cut loose, maximize all our powers, without any adequate or instant provocation. Thus our life is punctuated with alternations between drifting with the tide and rowing with might and main either with or against it, as the domestic horse now often cavorts and runs away in frantic terror perhaps from the most trivial cause, all because this psychic diathesis was stamped upon his stirp from the time he was a timorous, fugitive, rabbit-sized creature on to the present time. His timid, panicky disposition is due to the long ages when he was the victim and perhaps the chief food of wolves and other beasts of prey now extinct, when he was exposed to fires on prairies and steppes, and during the many thousand years when he was the chief quarry of the Loess hunters (a single tract of five or six acres near Solutré being covered with their remains, in some places " nearly seven feet thick " or " heaped in enormous piles." Buttel-Reepen, " Man and His Forerunners," 1913, pp. 96). As it was in this long apprenticeship to fear that the horse acquired his racing powers, which were also fear-made, so man in the far-back curriculum of his race developed the propensity to have excessive outbreaks of activity and woman grew prone to brainstorms of affectivity, and today many forms of convulsive seizures are derelict and distorted vestiges of the tempo and rhythm with which our ancestors reacted to types of experience which have now more or less ceased to exist although their cadence still remains in the soul. Here thus as in most strong manifestations of emotion there is an anachronistic element. We fear with not only all that we, but with all that the race has feared.

The same is true of man collectively. In panics, theatre-fires, shipwreck, earthquakes, tornadoes, as well as those that arise in business, social, political life, as Le Bon's psychology of the mob shows, feeling is immensely intensified in gregarious man by contagion, and if with Baldwin we accept social inheritance back of all tradition, we must again have final recourse to man's genetic history. For ages, compared with which the historic period is but some two seconds, on Haeckel's cosmic clock that measures the cosmic day as twenty-four hours, the savage tribes from which men sprung were exposed to conditions that compelled them often to suddenly leave their homes, perhaps after decimation and near extinction by their enemies, to migrate far and long and after hardships and dangers begin all over again, adjusting to a new environment, with only a remnant of all that they once were and possessed. They were often led captive, enslaved, almost exterminated, robbed of property, wives, children, liberty, their very rights, traditions, and folk-lore largely lost, so that those who survived had only too good reason to fear all the worst that can befall man. Primitive tribes have always been wolves to rival tribes. The very tradition of these brutal ages has perished, happily for man's self-respect but unhappily for our knowledge of his genetic history. Instead of not being able to think too highly of himself, as Hegel said man could not do, it is hard for it to enter his heart to think too meanly of his ancestors, in the forgotten ages of the relentless war *à l'outrance* of all against all. Only when the secrets of this lost record are revealed can we understand the ultimate reason why even civilized man after long periods of settled and orderly existence is liable to spells of upheaval and revolt against existing institutions, economic, social, political, religious. It is the call of the wild tribesmen in him to break away, renounce, demolish, burn what he worshipped and worship what he burned, strip off all convention, and try with abandon to get back to his original nature and start afresh. Hence the chronic fear which all who possess and would conserve have for levelling nihilistic spirits that would make a *tabula rasa,* as man in the past has so often done. Thus man is not very securely established in the settled orderly ways of modern civilization, and deep in his heart he knows that it may pass away as did the older types of classic days, and new races, perhaps now as rude as were the Teutons in the days of Tacitus, take up the torch when it falls from the exhausted hands of those who now carry it and bear it on to still greater heights yet undreamed of. Moreover there is always the possibility that society may turn turtle, topsey-turvy-wise, the first becoming last and the lowest highest. Thus beneath all our vociferous confidence in both the stability and the optimal character of things as they are, there is always an ambivalent and more or less unconscious fear that they may all be reversed. Only in the daily fluctuations of the stock market quotations have we any adequate registration of alternating fears and hopes pertaining to a very special aspect of communal life, but the same delicate oscillations are always going on in every field in which man has created worths and values, and the vicissitudes of his racial history contribute a factor to all such hovering oscillations between confidence and distrust concerning everything that is most prized.

(II.) *Pavor nocturnus,* on which a voluminous literature has accumulated during the last twenty-five years, is of great interest to geneticism. It is most common between the ages of two or three and seven or eight years, although it may occur earlier and may follow puberty. The child wakes suddenly from a deep sleep, pale, or more often flushed, with perspiration, throbbing heart, cries of terror, eyes fixed as if on some imaginary object, or rolling wildly, in search of the cause of its fear, and muscles tense, perhaps almost convulsed. It rarely occurs at the beginning of sleep but in the early part of the night when sleep should be deep. It is rarely repeated the same night but there may be increasing nervous tension for a series of days or even weeks, culminating in an attack. The words uttered are very often incoherent and unintelligible. The arms are often thrust out as if in defense. The whole attack lasts perhaps from ten to thirty minutes before the child is fully awakened and soothed. It can rarely tell much of what it feared, if indeed it usually has a specific object of fear. It falls asleep again, tired out, and in the morning has very commonly forgotten all about it. The fact that it comes on after deep sleep has set in, distinguishes it from the starts that may come to us all in the sudden falling to sleep, yet the two groups of phenomena may be akin. Beyrand thinks that the dreams are more often of animals, big and small, and next of attacks by other children or by adults, and that they tend to re-occur at the same hour on different nights. Hypnophobia may result. Some ally it to epilepsy, eclampsia and eclipsis. Braun insists on a neurasthenic basis and thinks it is most common with a high degree of irritability. For Rey (on a basis of thirty-two cases) stoppage of air-passages by bedclothes or adenoids plays the chief rôle. Freudians ally it to sex orgasm. Steiner, Werthauser and Henoch think it a true disease, while West, Ringer and Souchut deem it a reflex. Seelacher (17 cases) thinks it most often due to bad blood and anemia, although constipation and dentition are factors. Boncour thinks symptoms of meningitis are very common and deems its attacks very grave although they come to the knowledge of the physician for the most part incidentally. Little even describes cases of *pavor diurnus,* Soltman thinks optical hyperasthesia a prominent cause, although he emphasizes education, heredity and chronic disease. Worms, alcohol given as medicine or taken by the mother or nurse, chlorosis, chorea, anxiety, indigestion, and many other factors have played rôles in etiological theories. Goodhart (37 cases) deems it a nightmare, although Coutts thinks the latter a reflex, while *pavor* is of central nervous origin. Incontinence and somnambulism often go with it. As to whether it is of somatic origin, according to the Lange-James theory, or ideopathic, *i. e.,* due to dream hallucination, or delirium, Silvermann wisely concludes that these theories represent two forms, both of which may occur, and thinks the day has passed when we can assign it to any one cause, organ, or function. The literature shows that it may at least co-exist with more and more types of disease. The child's general condition and degree of fatigue may have much to do with it, so that an experienced observer can often forecast its onset, and bad physical conditions of any kind may not only precede but follow an attack. It has even been said that *pavor nocturnus* is highly generalized because an infantile form of insanity.

In this chaos of description of cases and partial theories it is difficult to attain any unitary conception of this syndrome, and there is even yet, on the whole, more divergence than convergence of views. If the facts in this fascinating field can ever be correlated we must have a theory of sleep and must consider the relation of these phenomena to man's phylogeny, to both of which clinical medicine is at present strangely indifferent. Here it is at least certain that the explanation must be sought although in the present state of our knowledge this must remain far from complete. Sleep, whatever else it is, begins in a retirement from reality, in Janet's sense, to what Bleuler calls the autistic level. Its normal progress is on down the phylogenetic levels. Hypnagogic phenomena are vestiges of ancestral types of mentation. Old tendencies, repressed in waking, are set free. We indulge old and perhaps infantile wishes and propensities. Sleep is thus as devolutionary as is true Jacksonian epilepsy. As the higher levels successively fall asleep the inhibitions which they always exercise when alert upon the next lower levels are removed. Only the lower centers, digestive, circulatory, respiratory, never sleep. In going to sleep, then, there is a lapse in the direction of embryonic conditions, which infants tend to assume even in posture. Going to sleep is thus a very complex process, taking, according to some studies, over an hour to reach its highest degree, even after the subject seems to others asleep. Ideally we can conceive it as a glide down an inclined plane, across many evolutionary strata, and it is a long glide from maximal alertness to the most complete slumber, the highest and latest acquired functions going first and the more primitive last, as far down as the "diurnal function" extends. Morals, propriety, all the restraints of the environment, are lost with the eclipse of the senses, and the sleeper becomes an automaton, the vegetative functions of life predominating. Thus the sanest, wisest, best, may yield for a time to the most insane, bad, foolish impulsions, on the way to or from complete sleep. Instead of going to sleep ideally, however, there are usually one or more drops, as if we were going down steps or a ladder, or falling across lost stages, instead of sliding down a smooth plane. There are perhaps abrupt changes of association; waking channels are closed and others opened suddenly; new balances between innervation and inhibition are abruptly struck. In this way the effect of ancestral experiences, usually kept down by many old restraints, is set free. Propensities that have long slumbered in the unconscious regions of the soul break out, perhaps with almost fulminating intensity. Sometimes the very strongest and worst of these eruptions bring momentary pleasure that the censor that watches even dreams and is so hard to escape is circumvented by many an intricate mechanism devised for that purpose.

How, then, can we explain these shocks, starts, spasms, and the fears they inculcate? To this end we must realize that normal sleep is a product of long and painful evolution. Some, if not most, of man's forbears, like many existing apes, may have been at least semi-nocturnal in their habits. In unprotected primitive man the habit of long, sound sleep would have been dangerous, exposed as he was to animal and human foes, so that safety would require promptness in waking for efficient defense. Night, too, was peopled with imaginary objects of dread, that since science, which means prevision, did not exist, and laws of nature were unknown, gave a feeling that almost anything might happen at any time. Hence sleep must have been with reservations, as it were, as a nurse sleeps, or on arms, rather than with abandon. Man slept as we do now when expecting calls to awake. His mind was attuned to respond quickly to danger signals. In the open, weather changes of various kinds would perturb sleep. In the hunting stage real and perhaps dreamed of wild beasts that prowl by night, in the pastoral stage dangers to flocks, if surrounded by human foes, perils of ambush or night attacks, could not fail to lessen its intensity and often interrupt sleep. Night was the most dangerous, instead of being, as it is in our sheltered condition, the safest time. Sudden alarm in darkness meant intense, wild, defensive movements, for danger that might be mortal was imminent, but in the darkness with no knowledge of what or where it was. This condition is exquisitely reproduced in typical cases of *pavor nocturnus,* in which such old alarms seem to re-echo. The blind, aimless struggles and screams for help, represent the way in which our forbears responded to night alarms. In the victim of night terrors this mechanism is not entirely obliterated. His acts are the auto-kineses of fear, while the dread fancies connected with them are far more polymorphic than the movements. Perhaps indeed the psychic factor is secondary or absent, although low level shocks of themselves have great power to start up the psychosis of fear. Thus very likely those who think that *pavor* may develop on the basis of infantile convulsions are right. The earliest, most generic forms of convulsibility would hardly have a psychogenetic root unless it be in the form of long-forgotten racial experiences. Motor are far more prominent and older than psychic expressions of fear. Even the starts, so common as we begin to fall asleep, may best be interpreted as rudiments of defense from which the psychic elements have more or less completely vanished. The Lange-James theory may hold for the race but not for the individual, psychic fears having made the mechanisms, which far outlast them. We cannot too often realize that the environment has created man, that he is nothing but a complex set of records of the past of his stirp, which have been stored within him, and that of all these records motor patterns are the most important or persistent, whether they were shaped through the ages by distance or by contact septors. The neurotic child, on lying down and going to sleep, breathes superficially; his blood is insufficiently oxidized, and he starts up with symptoms of dyspnoea, when, or as Little thinks, just before, his sleep has reached its deepest state. In some cases, *pavor* seems to have an aura, and this may have varied forms. The occasional cause of its onset may come from within or without. The dread of bed and sleep is so great that the child is unable to sleep without a trusted companion nearby. The nervous child may drop off very suddenly and very deeply. The deeper forms of *pavor nocturnus* seem to occur when sleep has gone below the level where the conditions for dream formation are most favorable. We usually dream in rather the earlier stages of falling asleep or the latest stages of awaking, so that it is a characteristic of the transition stage to and from true and deep sleep. Motor patterns, especially the older and ancestral ones, lie below this level, and individual experience determines only the height of the level at which these patterns are actuated. In *pavor* we have to do with an older complex which is like a rudimentary organ which has not been differentiated, as it normally should have been, into more specific ones, like *e. g.,* gill slits that may persist in the foetus till and even after birth instead of being made over into those portions of the eye, middle ear, larynx and thyroid, which they later help to constitute. It is thus these old motor patterns, beginning perhaps in infantile convulsions, and then passing into *pavor nocturnus,* and later into nightmare, epilepsies, and the host of other shock disorders, which Groenninger and others have attempted to correlate as a peculiar diathesis. Children normally awake slowly from lower to higher levels, and if this sequence is inverted, as it is by a sudden outer stimulus, and especially if this experience be repeated and a given level is often awakened by the one above instead of that below it, we should have shock during the ascending series of states of awakening, or perhaps a summative series of shocks from above downward, so that the waking series would occur in an order inverse that of the normal. Hence the sudden arousing of children favors *pavor* symptoms. Sleep does not very long persist at its greatest depth, but, as experimental studies show, grows less through a number of the later hours of the night, so that in this sense we may be said normally to require hours in which to awake, while usually only from one to two hours are necessary for sleep to attain its maximal depth. In other words, the curve of falling to sleep is steeper than that of awaking from it. To awake suddenly causes a shock a little like that of being reborn or thrust without warning into a new world where all is strange, and every shock and start, the faintest as well as the most violent and convulsive, is partial awakening. The closing in, downward process of sleep is not only arrested but reversed so that in neurotic states the descent into the valley of Morpheus is not a steady slide but more like that down scenic railroads in "White Cities," with many plunges and rises which respectively increase and diminish momentum, while waking, on the other hand, is a slower progress up the same road. Two traits often specified in the literature of *pavor nocturnus* are that its victims are prone to be precocious or retarded in their mental development and also to be of an affectionate or clinging disposition. Both are significant, the latter because *pavor* is at the same time both cause and effect of a passive temperament. It goes with timidity and lack of self-confidence, and it has been suggested that a reiterated experience of such seizures by their own automata predisposes to fancies of being seized by others, and may become the *Anlage* of fictive tales of being robbed, bound, ravished, and of a Masochistic disposition generally. As to precocious and arrested children, if falling to sleep reverses in any sense the course of ontogenetic or phylogenetic development, while waking rapidly repeats the salient features in the life of the individual, the race, or both, it follows that any obstruction or undue facilitization of the *élan vital* into the stages of descent into and ascent from the valley of sleep, would be deleterious, and that both ways would need special safeguards, on the importance of which pedagogues and paedologists have for generations laid the very greatest stress.

Again somatogenetic confirms and ideogenetic *pavor* confutes the Lange-James theory, for in the latter the dream causes the convulsive movements, and not the reverse.

Primitive man lacked foresight, knew little of the laws of nature, was unprepared for emergencies and so was exposed to the unexpected, to surprise, dreaded the new, and above all was liable to supernatural terrors, also to attack from real and fancied enemies, human and animal, as well as to dreadful monsters created by his own imagination, was spied on, ambushed, sprung at, appalled by tropical lightning and crushing thunder and long and doubtful struggles with great beasts of prey to see who should be lord of creation, must often stake his life, if not that of his tribe, against that of human foes, was the victim of countless accidents against which civilization has learned to protect us, might have instant calls to do his uttermost and use all his reserve power to escape or resist. The wager of battle was a hand to hand conflict, in which his every sense of muscular strength, skill, quickness, endurance, was put to the severest test. Have all these accumulated experiences left no trace upon us as descendants? Can we suppose that, had man and his predecessors always lived sheltered lives, with no sudden alarms, he would today have been as predisposed to convulsible states, sleeping or waking? Is it not rather the part of common sense to conclude that all experiences like the above constitute together the chief factor in man's *Anlage* of response to all explosive states and brainstorms, and that from the spasms of childhood and man's starts on falling to sleep to nightmare, night terror and epilepsy, and all the other shock neuroses and psychoses, there was thus some preformation in our constitution, and that indigestion, smotheration, fatigue, and all the other exciting causes are only occasions which set off a long pre-existing and inherited mechanism, least compensated for in neurotics and in victims of defective inheritance, and that the form which the outbreak takes is an accident of age, individual, or more recently ancestral, experience, or the order in which the repression exercised by the higher centers, is removed, or which of them is remiss either in sleep or in devolutional diseases.

In the large and heterogeneous class of convulsive diseases there seem enough similarities to warrant us in grouping them together, as ought to be done from the genetic standpoint. Steps toward defining a larger generic conception of epilepsy have already been taken by Davenport and Weeks, while Rosanoff and Orr in their study of Mendelian heredity in the endogenous cases of 72 families with 1,007 offspring took another important step in this direction. These and a few other similar studies so far made indicate that at least many diseases can be traced back to some very generalized form, as can many animal species that have gone far on the way of differentiation. Such morbid symptom groups must be traceable to the lack of some factor or the loss of one of the many unit characters or determinants. Paroxysmal spasmodic traits of disease, of which we have taken *pavor nocturnus* as a paradigm, we may conceive originated in the sudden extrinsic calls upon organisms to act with the greatest promptness and energy. This would of course at first tend to speedy and great exhaustion. The power to act thus, however, was of very great selective value, and so the power to store and suddenly discharge energy ensured the survival of those who could do it. At first only external influences caused these detonations such as urgent necessities for flight or fighting, to resist rape, robbery, or other forms of attack, and all the movements involved were at first purposive. Again, the stimulus to these intense reactions might be slow and cumulative, so slight as to be unconscious and produce no response for a long time while motor energy was slowly accumulating, till at last a crisis of reaction came which changed the environment. If the outer evocation to act was too great or too frequent, the individual would suffer exhaustion and collapse. If on the other hand the outer provocation did not come, the accumulated surplus energy held in reserve would at a certain point of high tension be set free by endogenous stimuli. Thus the convulsive phenomena of *pavor,* epilepsy and perhaps the more specific affections like trismus, tetany, Thompson's disease, chorea, tics, and the rest would gradually come to be set off in a genetically secondary way by unusual stimuli within the body instead of, as at first, without it, especially from exceptional impressions arising from the digestive, circulatory and respiratory tracts, and many others, prominent among which are sensory, nervous and psychic excitants. If the results of all these drastic ancestral experiences be not entirely lost it would seem that some of them would be found to be co-factors in a large class of hyperkineses, and that many of the motor patterns we glimpse here must be regarded as scars of ancient fears made when the great and fundamental emotions were far more intense and formative than they are now. We start perhaps automatically now because our ancestors were convulsed by terrors. We shudder or experience a shock and then feel fear while of old the feeling of fear was primal and created the physiological forms of its expressions, most of which are now worn down and rather purposeless relics of what were once far more definite and effective. Fear made mechanisms and then retreated as it could do because it had done its work so well. The phylogenetic order was first the great primal fear, moulding plastic functions and even protective organs to its service. Then, having greatly enhanced safety, the fear ebbed. Third, a changed environment made the old types of defense obsolete, but they persist, in a sthenic if somewhat now incoordinated way, and when they are called into action now they evoke a faint phosphoresence of the old primordial feeling. This later stage, and it only, conforms to the Lange-James rubric. The old motor patterns which pristine fears once made and used, which have such great power of persistence in us all, but especially in neurotics and children, worn and rusty though they be, can still arouse to a degree the yet older feelings that originally fashioned them, and although their defensive value has long since passed away and they are often impediments, they remain interesting though confusing vestiges of a time when our forbears were in palpitating touch with a rugged fear-provoking environment which has now for the most part passed away.

Thus fear is phyletic flight or defense. No matter how refined we are, we fear in the same terms of the same old gross organs and functions as do the brutes. We fear in the heart, the lungs, the skeletal muscles, the brain, kidneys, stomach, and sweat because this was once necessary in order to regulate the temperature in violent effort. We breathe fast and deep because rapid oxygenation was necessary. Thyroid action increases in order to accelerate metabolism; the adrenals act to augment blood pressure. Glycogen, of which the liver always has a modicum ready for use, is consumed the most rapidly because it is the most available nutritive element, while on the other hand, the digestive and procreative systems are reduced if not positively inhibited in their function because in the intense activities of fight and flight they were *hors de combat* and could not add to the efficiency of the moment. Fear does, however, strongly tend to empty the alimentary tract, decreasing body weight and perhaps increasing agility thereby. Only those functions in which there was no instant help are checked, and all others are toned up to their highest pitch. We clench fists and teeth because our predecessors clawed and bit, and we have the tensing-up phenomena of heart, lungs, muscles, because all these phenomena were so long and firmly associated not only in the life of the individual but phyletically. Thus the manifestations of fear in us today are of prehistoric and prehuman origin. Compared to the age of his stock or stem, man is just born, and while he has evolved certain functions of intellect since he has attained man's estate, his far more basal feelings are archaic and have developed no new organs. Primitive man and his ancestors too had great vitality, and even today savages survive severe operations almost incredibly. This fact is of profound significance for understanding the genealogy of fear, for if our predecessors had died of every slight trauma or injury, they would not have transmitted its lessons of wisdom and caution, so that their posterity would not have known how to fear aright, and both heredity and tradition would have had less value in making them do so. Again, a skunk, a venomous serpent, a turtle or an armadillo, the self-preservation of which is due but in a slight degree to its strength, agility or wisdom, would naturally develop but little temibility. Emotions now touch very lightly the same keys which once, pressed hard, played the loudest and most barbaric antiphone of behavior to experience in all the history of life.

Finally now, with all this complex and varied mechanism of fear so well developed, as it had been originally by the emotion itself, fear that is prevented from using its own organs is often exhaustive to the point of danger. Caged animals, terrified, but unable to fight or fly, show more depleted brain cells than those that can react normally, so that some have thought that emotion was inversely as motion, from which it sprang. To have the blood flushed with secretions from the thyroids, adrenals and liver, to have it charged with oxygen and at the highest pressure, the heart throbbing, respiration rapid, and yet to be able to do nothing definite, may be perilous, and at any rate is wearing like running the machinery of an auto with the throttle thrust forward, and, as Crile, in his excellent article (Proc. of the Amer. Philos. Soc., 1912, pp. 76-90), well suggests, to teach a patient the danger of this state of fear and thus to check it, is now entirely feasible. Still it should not be forgotten that such inhibitions are the prerequisites of all processes of sublimation and that if we had always feared convulsively we never should have come to fear rationally. The former, though dangerous to the weak, is an opportunity for the strong. Indeed at bottom the tension of fear that does not at once find vent with abandon in outgrown brutish ways, is the same tension which originally made these old low-level functions. Repression is the first call to go higher and find or make a better mode of expression. If man had always continued to exhaust himself, dancing out his savage fears of epilepsy, pestilences, spectres, etc., he would never have found more scientific modes of allaying these fears. The great and strong must fear greatly and strongly but refrain from the more instinctive forms of expression, for from this long-circuiting upward have sprung most of the epochful emancipations of man's estate from its long oppression by fears.

I cannot find in the literature on *pavor nocturnus,* covering hundreds of cases, a single study of the subject (especially from the ideopathic side) that begins to be up to the methods of modern scientific psychology. Like, to some extent, fear, in general, *pavor* lacks the keen analysis and observation which Jackson gave to epilepsy and Freud to sex. Very few clinicians, and much less physicians have ever seen or even studied with anything like the resources now available many or probably some of them any actual cases, but their records are mostly made up of observations of nurses, parents, attendants, etc. The somatic trend of modern medicine, added to the doctor's solicitude to relieve and cure, have prevented the effort to get from children at the critical moment of awakening such light as they might be able to shed upon the imagery or deliria or other psychic content of the seizure. A single case, long and intensively studied, with all the resources now available, would be worth scores of the accounts or compilations printed in medical journals and would mark a new epoch in the subject. It would contribute something of value to confirm and develop, or conversely to correct or refute the views I have ventured to express above. Some thirty years ago Helmholtz told physicians in an elaborate address that they could not and would not think. Since then medical science has developed by leaps and bounds but almost entirely on the physiological, chemical and bacteriological basis. In the field of mental medicine physicians are still not only as unable or unwilling to think as Helmholtz charged, but they do not think it necessary to think. A symptom group is best explained by finding or conjecturing some center, while in fact the more psychic a phenomenon is the less likely it is to have any sharp localization. Then comes the professional horror of anything that seems to favor mind-curism. In the field of *pavor nocturnus* in particular, there is very little literature that surveys the entire subject or field or attempts any synthesis. It is an eternal discussion of definition, nomenclature, organic or functional disorder, which is assumed to be at the basis of everything, while all psychotic phenomena fall in the blind spot of the medical eye because these topics are very rarely taught in medical schools.

AN EXPERIMENTAL STUDY OF STUTTERING*

By John Madison Fletcher

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I. Introduction

The aim of the present thesis is to offer a psychological study of stuttering. This phenomenon is a familiar one but it has received scant notice. The only scientific attention that has been paid to it has come from medical authorities. To the medical fraternity it has been known at least since the days of the Egyptian hieroglyphics (19); but from the medical point of view there is yet no concensus of opinion as to its nature or its treatment.

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* From the Psychological Laboratory of Clark University.

In reference to the question of incidence no complete statistics are available. On the basis, however, of certain careful estimates it is claimed (24; 38-39) that there are approximately a half million speech defectives in the United States. This number is much in excess of the number of the blind, the deaf and dumb, the insane, or the feeble-minded.

Defects of speech are considered to be of more scientific and practical importance in Germany than in the United States. In 1886, under Minister of Education von Gossler, the government began activities on behalf of these defectives which have not ceased (72; 112 ff.). Owing to the efforts of the late Dr. A. Gutzmann and his son, Dr. H. Gutzmann, the University of Berlin has been the source from which teachers from the various cities of Europe have secured training for service in the schools. Japan, through the activities of Isawa, has also taken an interest in the problem. In the United States the subject has not received its due scientific attention, while the practical handling of the problem has been almost totally neglected. The victims are not even protected from the charlatans who prey upon them.

It is hoped that the present research will show materials for laboratory use in the study of other problems of general psychology. Advantage could, of course, not be taken of all the ramifications of the problem; nevertheless the clinical cases used in our investigation seemed constantly to afford excellent materials for the study of such topics of general psychological interest as the physiological expression of emotions, the phenomena of inhibition and association, the *Aufgabe* consciousness, communication, motor co-ordination, voluntary control, attention, etc. Our cases presented a variety of mental conditions that are capable of being repeated at will, conditions which it would be impossible to produce with normal subjects under the usual laboratory conditions. Yet, unlike the insane or the feeble-minded, they possess an intelligence that is normal; and hence they are able to carry out detailed instructions and, if trained, to introspect.

This study, however, is not an attempt to fill a lacuna in any psychological system; it is a frank appropriation of the ordinary methods of laboratory psychology for the study of a problem that seems to be not only a legitimate one for psychology, but also a problem that is of sufficient value to justify the research. It is to be hoped that the practical value that the problem happens to possess will not rob it of its scientific interest for those who hold to the Wundtian notion of the province of pure psychology.[1]

The study seems to have turned out to be neither exclusively functional nor exclusively structural. It was found necessary to view the phenomenon of stuttering from both aspects, a fact which cannot, of course, be said to make of it a peculiar problem. It also seems to lie somewhat between the fields of normal and abnormal psychology. The subjects are not to be distinguished physically from other people; they are of normal intelligence; their thought processes are logical. In the linking of ideas with the motor processes of speech, however, they are not normal. Arising out of this inability there are to be found certain other associated abnormal conditions that are to be indicated in detail later.

It is desired that the present study will help toward the narrowing of the problem to more convenient proportions. At the present stage it is necessarily comprehensive. In order to reduce it to as narrow a compass as possible the following main problems have been set: *1.* to describe stuttering in terms of its physiological manifestations; *2.* to enumerate and describe the associated mental conditions; and *3.* to ascertain, if possible, the part played by these mental states in its causation.

II. Differentiation and Definition

In order to avoid confusing stuttering with other forms of speech-defects it will be necessary to adopt a differentiation of the several groups of defects, which for present use will be as follows:

1. There is a class of speech-defects resulting from diseases or lesions in those portions of the brain that have to do with the function of speech. This type of defect is known as aphasia. It may be sensory, motor, or associative, according as the region of the brain affected is a receptive, emissive, or associative center. There are many types of this defect, such as aphemia, agraphia, alexia, apraxia, etc. It is an organic disorder due to the degeneration or destruction of brain tissue; it is therefore a form of dementia and hence is a problem for psychiatry rather than for psychology. It will not be taken into account in the present study.

2. Secondly, we have a class of speech defects to be designated in this connection as "stammering." Schulthess (24; 27) in 1830 recognized this as a distinct type of defect, and differentiated it from stuttering by the adoption of the two terms *"Stottern"* and *"Stammeln,"* which are still in use in Germany. In England and America the terms "stuttering" and "stammering" are used synonymously, with the result that a confusion exists as to the two conditions designated by these terms. To avoid this confusion it has been suggested that technical terms be adopted to designate the two fundamentally different defects. Scripture (127) proposes the terms "hypophonia" and "hyperphonia" to designate stammering and stuttering respectively. "Hypophonia," he says, is "subenergetic phonation," while "hyperphonia" is "superenergetic phonation." This terminology is open to the objection that a differentiation on the basis of the amount of nervous energy employed in speaking does not harmonize with the facts. Stuttering is not merely speaking with an excess of nervous energy; nor does stammering consist merely in speaking with a less amount than normal.

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[1] W. Wundt, Ueber reine und angewandte Psychologie, *Psychol. Stud.,* V. 1909, 1-47.

Dr. Makuen has proposed terms that more nearly describe actual conditions (98). For stuttering he proposes the term "dyslalia," or difficult speech; for stammering he proposes the term "pseudolalia," or incorrect speech. However, between the difficulty of securing the general adoption of a new terminology and that of differentiating the terms already in use, the writer has chosen the latter alternative mainly because this does not necessitate a break with the terms in use in the German literature of the subject, which is the most extensive.

Stammering is mispronunciation (107; 567-568). The stammerer, unlike the stutterer, can always speak; but his speech is incorrect. This type of defect is found in two forms, namely, (1) the anatomical, and (2) the developmental. The anatomical stammer is due to a malformation of one or more organs of articulation, as the lips, tongue, teeth, hard or soft palate, the nasal or pharyngeal cavities, etc. The developmental stammer has no such physical cause, but is due to the incorrect functioning of the organs of articulation resulting in certain cases from immaturity, as in the "baby talk" of normal children, in whom the condition is not regarded as pathological unless it is prolonged beyond the age of five or six years (24; 22).[2] The developmental stammer, which is characteristic of all children at an early age, arises from incomplete perception of vocal sounds and from lack of co-ordination of the muscles of articulation. Lisping is a common form of stammer and consists in the substitution of *th* sounds for those of *s* and *z*. The organic type of this defect is due to the shortness of the lingual frenum, or "tongue-tie."[3] Stammering possesses no special psychological interest and will not be considered in the present study.

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[2] Colman (22; 454) gives an extreme case of "baby talk," or infantile stammer. This child in attempting to say the Lord's Prayer spoke as follows: "Ouë Tade na ah in edde, anno de Di nã, I tidde tah, I du

3. Thirdly, there is a class of defects to be designated as "stuttering." This group is distinguished from the foregoing types mainly by its intermittent character. Stuttering may be called a temporarily appearing inability to begin the pronunciation of a word or syllable (107; 567-568). The capacity of the stutterer to speak seems to be related to certain mental attitudes or states of mind. It is this characteristic that gives the subject its psychological interest.

The evidence for the diagnosis of this defect as primarily mental seems to be accumulating. It was formerly thought to be due to permanent anatomical anomalies, which were amenable to surgical treatment (28). Wedge-shaped portions were cut from the back of the tongue; the hypoglossal nerve, the lingual frenum, and the various extrinsic and intrinsic muscles of the tongue were severed. The tongue was pierced with needles. Cauteries, blisters, and embrocations of petroleum, also inoculations of croton oil were administered. Tincture of rectified alcohol, peppermint oil and chloroform were applied. Wooden wedges were placed between the teeth. Smoking was recommended as a sedative to the vocal cords. The above and various other remedies were devised on the assumption that the difficulty was organic and peripheral (103). This theory gave place to the notion that, though the defect is not caused by gross anomalies, yet a permanent impairment of the nervous system is present. Kussmaul holds consistently (86) to the view that stuttering is a "spastic co-ordination neurosis." This view is adhered to by many modern writers, especially by those who approach the subject from the medical point of view. Gutzmann (66; 211) holds that every stutterer is neuropathically af-

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de di on eet a te e edde, te ut te da ouē dade ded, e didde ouē tetedde a ne ahdin to te tetedde adase ut, ne no te tetate, ninne ut enu, to I ah te nini, ponē e dordy, to edde e edde, Amé."

[3] Scripture, in his recent book, *Stuttering and Lisping* (New York, 1912, 251 pp.), written subsequently to the completion of this paper, has attempted to change the meaning of the term "lisping" to include defects that are usually classified as stammering. See pp. 111 ff.

flicted. Scripture claims (125) that "stuttering is a distinct form of nervous disease . . . ; it can be properly and legally treated only by a physician."

There is, however, a tendency among many medical authorities to lay chief stress upon the mental aspects of the phenomenon, so that the third stage in the progress of its diagnosis may be said to be the one in which mental rather than physical conditions are regarded as primary. Makuen, for example (103), says that "many persons stammer (stutter) under certain conditions, largely because they think they will. All their past experiences with speech have combined to confirm them in the thought and it soon becomes a sort of "fixed idea." Langwill (88) believes that "stammering (stuttering) is essentially a functional affection,—a neurosis, the result of the defective working of the complex co-ordinated mechanism concerned in the production of speech, and not a defect of structure. Pathological lesions, therefore, need not be looked for." Gutzmann, in spite of his agreement with Kussmaul, as indicated above (p. 205), says (59): "In the psychology of stuttering we do not find the destruction of any separate fibres or special centers; also the connection between the sensory center and the ideational centers, on the one hand, and the motor center, on the other, are undisturbed. The stutterer simply does not succeed in making the co-ordination from the motor center to the organs well enough to produce the correct mutual functioning."

III. Physiological

Physiologically the function of speech involves the co-ordinated action of the three musculatures of (1) breathing, (2) vocalization, and (3) articulation. The aberrations from the normal, that are found in the stutterer's speech, in respect to these three functions, will be taken up in order.

A. Breathing

a. Historical. Until the methods of observation and palpation had given place to the use of instruments, such as those devised by Marey, Bert, Zwaardemaker, Rousselot and others (59), exact study of these functions was impossible. With some exceptions the earlier studies of breathing were primarily physiological; and they largely disregarded the mental influences which are taken into account by such studies as those by Mentz (105) and Zoneff and Meumann (149). Before abnormalities could be indicated it was necessary to establish the simpler facts of normal breathing, such as the difference between rest-breathing and breathing during speaking; the relations between thoracic and abdominal breathing, and the like. Halle, who recognizes the influence of mental states on breathing, but who does not attempt a definite correlation, was one of the earliest investigators in this field (73). He found in the stutterer:

1. Tonic or clonic spasms of the diaphragm in all cases;
2. Psychical alterations of the breathing curve both before and after speaking;
3. Poor economy of breathing;
4. Attempts at speaking during inspiration;
5. Withholding of the breath after speaking.

Gutzmann (66) originally accepted the findings of Halle but has subsequently changed his opinion. His more recent conclusion (66; 202) is that if one would avoid error one should disregard the influence of mental states on breathing changes altogether. This conclusion, however, seems in turn to be modified by his admission that certain states of mind can have an effect on breathing (64; 16). To admit that this is an unsettled question would have an important bearing upon the problem in hand, for, if mental states have no influence on the breathing, stuttering, in so far as it is a disturbance of breathing, must be essentially physiological.

Ten Cate, who was the first to make simultaneous records of thoracic and abdominal breathing-curves, found (18) that the stutterer is not below the normal in chest expansion; and that, though his expiration period is shorter than the normal, his lung capacity is not below the average. However, he finds that the stutterer's strength of inspiration is greater than that of expiration, which is the reverse of the condition found with the normal speaker.[4]

There are no complete statistics on the number of male and female stutterers; but estimates (24; 362 ff.) show that male stutterers preponderate over females in ratios ranging from

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[4] This last finding would seem to point to some permanent physiological difference that is characteristic of the stutterer. As bearing upon this point it is of interest to quote certain findings mentioned by Greene (44). Greene reports that in cases of forced breathing the strength of inspiration of women is 60 per cent. of that of men, and that of expiration is 66.7 per cent.; that in quiet breathing the strength of the inspiration 66.7 per cent. of that of men, and that of expiration is 63.25 per cent. In all cases, therefore, women are inferior to men in strength of respiration. Yet there are several times as many male stutterers as female. Strength of respiration, therefore, does not seem to be a determining causal factor.

2:1 to 10:1. The male preponderance is much greater in childhood than it is in adulthood; psychological, physiological, and sociological differences between the sexes have been pointed out as causes of this preponderance. Differences in breathing-types have been most frequently suggested. That there are sex differences in this regard seems to be granted; it is held, however (37), that such differences are not congenital but are due to the habit of constriction.[5]

b. Subjects. L. R. is a boy fifteen and a half years old, who dropped out of school while in the sixth grade because of his inability to talk. He is in good health, is well grown and well nourished. He is cheerful, energetic and of normal mentality. He began to stutter at the age of two without any known cause.

E. B. is a boy in the eighth grade of the public school, and in spite of his difficulty is making normal progress in his studies. He is healthy and of normal intelligence; his heredity, so far as could be ascertained, was negative for all that could be considered to form a neuropathic basis for his defect.

F. Q. is a boy 17 years old, of good intelligence, and of pleasing manner. He has had for some years a valvular disorder of the heart. His stuttering was said to have originated in an experience of fright. While playing on the street at the age of six he was accosted by a man who he now thinks must have been insane, and who asked to be directed to a certain place. When the boy attempted to show him the man grasped him by the hand, dragged him into some tall grass and left him. When the man went away the boy escaped and has stuttered since that experience.

P. A. is a boy 14 years old, normally intelligent, and in good health. He is somewhat rough, indifferent, headstrong, and unreliable. There are no significant facts of heredity, nor is the history of the origin of his defect known.

A. N. is a young man 22 years of age, of good intelligence, cheerful and friendly. He has good business qualities, and is of good habits. His heredity is negative, and the history of the origin of his defect is unknown. His stuttering is of severe degree; he is often unable to think of what he is trying to read when stuttering. His mind seems to become befogged under those conditions, and only the memories of feelings and strains are left behind. Even when not attempting to speak he reports that conditions of what appear to be "blank abstraction" come over him.

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[5] If permanent breathing-differences, due to constriction, are responsible for the differences in the numbers of male and female stutterers it would seem that where customs of tight-lacing do not exist there should be no such marked differences. As bearing upon this point the writer secured from Japan statistics concerning the prevalence of stuttering among 135,852 boys and 20.637 girls. These statistical data show approximately the same sex differences as are found in other countries. In one prefecture (Yamaguchiken) 5.06 per cent. of the boys are stutterers while only .16 per cent of the girls stutter.

W. N. is a boy 12 years of age, and is the son of a stutterer. The father reports that he himself acquired the defect by imitating a school-mate who stuttered. The boy is regarded by his teacher as being one of the best students in the school. He is in good health. In his stuttering he shows a tonic closing of the lips, the bottom lip pressing against the inner side of his upper teeth, while the chin is in a tremor. In reading, his eyes run ahead to detect the difficult words.

H. A. is a young man 24 years of age who dropped out of high school on account of his defect. His stuttering began in the fourth grade, and is attributed to the act of imitating a school-mate. He is intelligent, active, competent; he is the son of a physician, and he has excellent training as a machinist. His health is good, and his heridity is negative. His stuttering is worse in conversation than in reading aloud. Talking over the telephone is easier for him than any other form of speech. His stutter movements are strongly suppressed, being confined to the opening and closing of the mouth while the tongue is adhering to the roof of it. Certain consonants give him much difficulty.

J. L. is a young lady of 18, who is in the third year of the high school. On account of protracted illness, she lost considerable time from school while in the grades. On returning, she was nervously anxious to keep up with her class; and her stuttering began during this period. She reports that when she realizes that she is going to be called upon by her teacher to recite, her heart beats violently. In addition to the stutterer's usual ability to sing she also possesses the ability, not uncommon among stutterers, to speak in public and to participate in amateur theatricals without fear of stuttering. Her stuttering is confined to certain consonants, of which she is in dread. She too runs ahead in her reading to see if difficult letters are ahead of her.

H. D. is a young man, aged 21, who began stuttering at the age of 5 by imitating a cousin. He is in good health, and is physically and mentally vigorous. He was one of the best debaters in his high school, and was the orator of his class on graduation. Under the stimulus of speaking in public, he does not stutter, while in private conversation he is often unable to make himself understood. His stutter movements consist of clinching the jaws and holding them in tonic condition until a violent explosion of the attempted sound takes place. Often his mouth will open widely without effecting the sound desired.

The greater part of the data for the present study was secured from these nine subjects. Many other stutterers were examined, from whom we obtained a limited number of records bearing on certain points.

c. Method. The subjects were seated comfortably in a chair by the side of the table upon which stood the registering instruments; the instruments were hidden from view by a screen. Extraneous noises and other distractions were reduced to a minimum. Time-lines were recorded by means of a Jacquet chronometer. The apparatus was so arranged that the experimenter could manipulate it without moving from his seat, or being seen by the subject. For taking the breathing curves two Sumner pneumographs were used, one being placed about the thorax and the other about the abdomen. The registrations were made by Marey tambours on smoked paper attached to the drum of a kymograph. Signal keys connected with the kymograph by means of tambours were used to indicate when the subject was endeavoring to speak. Cards on which were printed various selections of prose and poetry were used as material for speech. When these failed, as they sometimes did, to evoke stuttering, the subject was asked to relate in his own language the substance of something he had read or to give an account of something with which he was known to be familiar. If this failed, strangers were brought into the room and the effects noted.

Attention must be called to the fact that, while in these experiments, methods of registration must remain constant, the stimulus used to provoke stuttering, which is the phenomenon to be studied, cannot remain constant for the obvious reasons that the same stimulus will not produce stuttering in all individuals, and that the same stimulus will not produce constant effects in the same individual.

Strictly speaking the asynergies of vocal utterance would include the disordered action of each musculature taken by itself, and in its relation to the other two musculatures (See p. 206.) In the study of breathing our chief emphasis will be put on the first aspect; whereas in vocalization and articulation it has been necessary for reasons to be indicated later to give chief emphasis to the second aspect.

d. Results. In general it has been found that stutterers present no permanent peculiarities of breathing unrelated to the function of speaking. This is in accord with the findings of ten Cate (18) and contrary to those of Gutzmann (66). It must further be said that there are apparently as many varieties of breathing peculiarities among stutterers as there are varieties of stuttering.

Detailed measurements of the breathing-curves were made by using a line drawn in the direction of the movement of the drum to be used as a line of reference from which measurements were made in millemeters to a point midway between the top of the expiration and the bottom of the inspiration curves. These measurements show that the mean variations and the averages are characteristically greater in the breathing-curve during stuttering than during normal speech. The total results of the cases measured are as follows:

Normal Speaking	1. Av.	26.2	mm.
	2. M. V.	3.79	"
Stuttering	1. Av.	35.6	mm.
	2. M. V.	4.62	"

The arhythmic character of breathing is evident from these data.

Many peculiarities of the curves, however, are minute; and do not lend themselves to tabular statement. (See Plates.) The characteristic aberrations may be grouped in three general classes:

1. *Retardations*
2. *Total misplacements*
3. *Interruptions*

These are characteristic both of inhalation and of exhalation. *1. Retardation* means the withholding of inhalation beyond the average time, and the attempt to speak when the lungs are comparatively empty. Exhalation may also be withheld; but this is not so characteristic. Exhalation for the stutterer is apparently more easily performed than inhalation; but it does not always result in the production of sound. Many cases show complete exhalation before the production of sound. In one case exhalation lasted eight seconds yet the attempt at sound was vain. Exhalation is aften interrupted by short, spasmodic inhalations. *2. Total misplacement* means the complete breaking up of the natural rhythm to the extent that inhalation occurs where exhalation should take place or vice versa. *3. Interruption* may occur in two ways; *a.* inhalation may be broken into by exhalation; or *b.* exhalation may be broken into by inhalation. These forms are frequently found; the speech accompanying such breathing is characterized by broken, jerky blurts of speech, appearing frequently in the form of repetition of the initial syllable of a word.

A study of the ratio of the time of inspiration to that of expiration was also attempted with the following results: On computing the averages of the records of each individual, and then the average of these individual averages, it was found that the fraction representing the inspiration-time divided by the expiration-time during normal speaking was .217 sec., whereas in stuttering it was .535 sec.; or, otherwise stated, the ratio of inspiration to expiration was found to be more than twice as great in stuttering as in normal speech. Stated in other terms it appears as follows:

Inspiration : expiration : : 217 : 1000 in normal speech;
" : " : : 535 : 1000 " stuttering.

Since the date of ten Cate's investigations (18), considerable emphasis has been placed upon the temporal relations between the thoracic and the abdominal curves. These relations have been expressed by such general terms as "ahead," "behind," "synchronous," and "anachronous." These terms are found to be inexact for tabulating results for the reason that if, for instance, we take the top of the abdominal curve it may be (1) ahead, (2) behind, (3) synchronous with, or (4) in opposite phase with the top of the thoracic curve if we use the latter as a curve of reference. Similarly, the bottom of the abdominal curve may sustain four relations to the bottom of the thoracic curve. Our tabulated results show that many records are characterized throughout by the fact that, during speaking, the crest of the abdominal curve is behind, while the bottom is ahead; whereas in rest there had been complete synchronism.

The following were compiled from typical cases of breathing curves of stutterers in an attempt to determine whether any definite temporal relationship could be established between the thoracic and the abdominal curves. In each case the thoracic curve is compared with the abdominal. Two points of the thoracic curve are taken, namely, the crest and the trough, and these two points are compared with the two corresponding points on the abdominal curve. Each of these

TABLE I.

(CREST.) SUBJECT *L. R.*

Showing the temporal relation which the thoracic curve may sustain to the abdominal. The figures in the various columns of the table indicate the number of times the temporal relationship, designated at the top of the column, occurs in the record whose number appears in the extreme left-hand column.

No. of Record	Ahead		Behind		Synchronous		Opposite	
	Speaking	*Rest*	*Speaking*	*Rest*	*Speaking*	*Rest*	*Speaking*	*Rest*
1...........	....	..	13	4	....	1	1	..
2...........	....	..	9	..	2	..	....	..
3...........	....	..	21	1	5	3	....	..
4...........	....	..	8	..	2	..	....	..
5...........	3	..	9	10	6	1	23	2
6...........	....	..	1	2	....	45	7	..
7...........	....	..	6	..	3	6	2	..
8...........	....	..	1	1	8	4	2	..
9...........	1	..	1	1	1	6	....	..
10...........	....	2	4	..	13	36	2	..
11...........	....	..	8	8	....	31	2	..
12...........	....	..	18	1	25	12	1	..
Totals.....	4	2	99	28	65	145	40	..

TABLE I.

(TROUGH.) SUBJECT *L. R.*

Showing the same as Table I above, but using the trough instead of the crest of the thoracic curve as a point of comparison.

No. of Record	Ahead		Behind		Synchronous		Opposite	
	Speaking	*Rest*	*Speaking*	*Rest*	*Speaking*	*Rest*	*Speaking*	*Rest*
1...........	1	..	10	2	1	1	....	..
2...........	....	..	8	..	3	1	1	..
3...........	6	2	11	1	2	..	....	..
4...........	1	2	1	..	3	..	....	..
5...........	1	...	24	2	2	6	6	5
6...........	....	..	....	..	1	45	17	3
7...........	2	..	4	..	3	5	....	..
8...........	2	..	2	4	4	2	2	..
9...........	3	..	1	..	1	6	....	..
10...........	....	12	....	2	9	19	1	..
11...........	....	..	5	3	1	34	4	1
12...........	10	3	9	..	22	11	....	..
Totals.....	26	19	75	14	52	130	31	9

points on the thoracic curve can sustain four general temporal relations to the corresponding points on the abdomina curve. These relations are, 1. ahead, 2. behind, 3. synchronous, 4. opposite in phase. The numbers of the record appear in the left hand column. The figures in the other columns of the table indicate the number of times the tempora relation, designated at the top of the column, occurs in the record whose number appears at the left.

TABLE II.

(CREST.) SUBJECT *F. Q.*

Showing the same as Table I above (page 213) with a different subject

No. of Record	AHEAD		BEHIND		SYNCHRONOUS		OPPOSITE	
	Speaking	*Rest*	*Speaking*	*Rest*	*Speaking*	*Rest*	*Speaking*	*Res*
1...........	....	..	19	15	....	..	1	..
2...........	1	1	9	23	2	..	3	..
3...........	....	..	11	15	....	..	....	..
4...........	....	..	9	5	1	16	....	..
5...........	....	..	25	7	....	..	1	..
6...........	....	..	9	14	2	11	1	..
7...........	....	..	8	..	....	2	....	..
8...........	....	..	8	..	....	1	....	..
9...........	....	..	4	..	8	..	....	..
10...........	....	..	6	..	....	5	....	..
11...........	2	..	8	..	....	2	....	..
12...........	....	..	6	1	1	4	....	..
13...........	....	1	8	5	1	..	....	..
14...........	2	..	5	1	2	..	....	..
15...........	....	..	5	..	2	1	....	..
16...........	....	..	2	3	2	5	1	..
17...........	....	..	24	..	....	..	....	..
18...........	....	..	13	..	4	..	5	..
19...........	....	..	11	2	....	1	2	..
20...........	....	..	12	..	2	..	1	..
21...........	1	..	12	..	....	..	....	..
Totals.....	6	2	214	91	27	48	15	..

Tables I, II, and III show the following characteristics:

I. The temporal relations of the crest of the two curve do not agree with the temporal relations of the bottom. (Cf., e. g., the crest and trough of Table II.)

2. The character of the breathing curve varies with the individual. (Cf., e. g., the totals of Tables I, II, and III.)

TABLE II.

(TROUGH.) SUBJECT *F. Q.*

Showing the same as Table II above, but using the trough instead of the crest of the thoracic curve as a point of comparison.

No. of Record	AHEAD		BEHIND		SYNCHRONOUS		OPPOSITE	
	Speaking	*Rest*	*Speaking*	*Rest*	*Speaking*	*Rest*	*Speaking*	*Rest*
1...........	5	..	....	12	10	7	....	..
2...........	....	4	8	6	3	10	....	..
3...........	....	1	....	14	11	5	....	..
4...........	4	..	5	12	....	10	....	..
5...........	2	..	11	..	2	10	....	..
6...........	1	..	7	13	....	2	....	..
7...........	2	1	3	..	3	1	....	..
8...........	7	..	....	..	2	..	....	..
9...........	12	..	....	..	....	..	....	..
10...........	6	1	....	..	....	4	....	..
11...........	4	..	1	..	4	3	....	..
12...........	4	..	....	1	....	3	....	..
13...........	4	2	2	..	1	3	....	..
14...........	10	..	....	..	....	..	....	..
15...........	8	..	1	..	....	1	....	..
16...........	7	1	....	..	....	1	....	..
17...........	22	..	....	..	....	..	....	..
18...........	8	..	3	..	....	1	....	..
19...........	6	1	1	..	....	3	....	..
20...........	8	..	4	..	1	..	....	..
21...........	8	..	3	..	....	..	....	..
Totals.....	128	11	49	58	37	64	....	..

3. The individual records of each subject present marked peculiarities at certain times. (Cf., e. g., record 6, Table I, trough.)

The abdominal curve usually shows slighter fluctuations than the thoracic. The former frequently assumes a tonic condition, which has often been described as "cramp" though certain objections have been raised (74) to the use of this term. In many cases the thoracic and abdominal curves tended to approach each other, often to the point of touching; and this would sometimes continue throughout the speaking interval.

With certain subjects (one in particular, *L. R.*) there was found on certain days a complete point-for-point opposition of phase, such as Janet (81; 256) found to exist in hysterical paralysis of the diaphragm. This phenomenon appeared during the speaking interval only.

TABLE III.

(CREST.) SUBJECT *E. B.*

Showing the same as Table I above (page 213), with a different subject.

No. of Record	AHEAD		BEHIND		SYNCHRONOUS		OPPOSITE	
	Speaking	*Rest*	*Speaking*	*Rest*	*Speaking*	*Rest*	*Speaking*	*Rest*
1...........	....	1	17	24	....	..	....	..
2...........	1	..	10	10	4	9	....	..
3...........	....	..	17	12	....	..	....	..
4...........	....	..	13	11	....	5	....	..
5...........	....	..	13	9	6	1	1	..
6...........	....	..	15	18	3	..	....	..
7...........	....	..	22	23	....	..	....	..
Totals.....	1	1	107	107	13	15	1	..

TABLE III.

(TROUGH.) SUBJECT *E. B.*

Showing the same as Table III above, but using the trough instead of the crest of the thoracic curve as a point of comparison.

No. of Record	AHEAD		BEHIND		SYNCHRONOUS		OPPOSITE	
	Speaking	*Rest*	*Speaking*	*Rest*	*Speaking*	*Rest*	*Speaking*	*Rest*
1...........	....	8	12	..	....	6	....	..
2...........	....	6	7	9	3	9	....	..
3...........	6	18	1	..	2	3	....	..
4...........	....	..	13	10	....	3	....	..
5...........	....	..	28	..	1	..	....	..
6...........	5	4	1	..	8	14	....	..
7...........	1	..	16	22	2	1	....	..
Totals.....	12	36	78	41	16	36	....	..

The thoracic seems to be more sensitive to mental disturbance than the abdominal curve. The latter sometimes remains stationary while the thoracic curve shows a uniform upward tendency or else considerable fluctuation.

In many cases breathing disturbances appear before the speaking interval, and also subsequent to it. These after-period disturbances have continued as long as eight seconds.

Breathing abnormalities seem to tend toward becoming stereotyped in certain forms for certain individual stutterers.

These individual peculiarities are numerous and varied in character. They often appear as temporarily adopted expedients to help out in the beginning of speech, just as the stutterer will often be found to start himself speaking by uttering a vowel sound like "ah." Breathing peculiarities of this kind are too numerous and varied to admit of classification.[6]

B. *Vocalization*

a. Method. The abnormalities of vocalization do not admit of being studied in isolation from the other physiological processes of speech as well as those of breathing. The aberrations to be studied under this heading will be the asynergies between vocalization and the other processes of speech, particularly that of breathing. This seems to be one of the most characteristic physiological manifestations of stuttering. The writer is not aware of any other attempt to study the relation between breathing and vocalization except by the use of the signal key, which is an inaccurate method.

It was not thought necessary for the present study to secure minute records of the pitch and quality of the voice. It was deemed sufficient to employ an apparatus which would faithfully record when the voice was sounding, and which at the same time would not be responsive to the breath. After the trial of several instruments the Rousselot microphone was found to be best suited to our purpose. The breathing records were made simultaneously with the voice, and on the same kymograph. It was therefore necessary for the drum to turn slowly; hence the stylus connected with the microphone shows only a rapid up-and-down movement during the period of vocalization.

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[6] Gutzmann's classification of breathing abnormalities is as follows:

I. Primary disturbances
  a. Too frequent inspirations; *b.* prior postponement; *c.* clonic and tonic spasms; *d.* temporal opposition of costal and abdominal breathing.
II. Secondary disturbances
  a. Spasms of breathing dependent upon spasms of the organs of articulation.

The writer has found examples of all of the above disturbances, and in addition has found disturbances which may be classified as the opposite of I, *a.* and *b.*

Ten Cate (18) finds:

1. That rest-breathing curves of the stutterer are like those of the normal person.

2. Disturbances of speech-breathing are always present in speech defects.

3. The normal anachronism of speaking-breathing is broken up in case of the stutterer.

4. Inspiration curves are steeper in the stutterer than in the normal individual.

The writer's findings corroborate points *1*, *2*, and *4*. but not 3. The findings reported above have not agreed with the claim that anything like a permanent synchronism between costal and abdominal breathing is to be found in the speech of the stutterer.

b. Results. It is to be remembered that the characteristic normal rest-breathing curve shows inspirations and expirations of approximately the same length, and that the thoracic and the abdominal curves are approximately synchronous in phase. When normal speech begins, the expiration period is greatly lengthened and the inspiration period is greatly shortened. Also normal speech shows vocalization at the initial point of expiration. Radical departures from these conditions occur in the stutterer's speaking interval. Parallel with the misshapen breathing curves, already discussed, the microphone enables one to locate and measure the periods of vocalization. The aberrations so far noted are of two general kinds: *1.* misplacement, and *2.* faulty utilization of voice. Under *1.* will appear frequent cases in which the subject is found to be vocalizing when both his thoracic and abdominal curves indicate inspiration, and still again when his thoracic and abdominal curves are out of phase.

It may almost be said that faulty utilization is invariably present. This lends itself to a sort of quantitative statement. By the selection of a series of type cases from normal and abnormal speaking, the following averages were secured: In the abnormal type, the expiration interval averaged 24.6 seconds. The vocalization interval occupied 9 seconds of this,—which means that 36.5 per cent. of the expiration interval was utilized in vocalization. The rest of this interval was taken up by aphonic disturbances such as are shown by the pneumograph.

The normal records, on the other hand, showed that of an average expiration interval of 26.8 seconds, 25.6 were utilized in vocalization. Or with normal speaking there appears an efficiency of 90 per cent., as compared with an efficiency of 36.5 per cent. in the case of the abnormal.[7]

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[7] It may be said that the voice of the stutterer often seems to the auditor to be husky and suppressed,—indicating a tension of the throat muscles, and an improper functioning of the muscles in control of the vocal cords. The husky sounds of the stutterer's voice often gives one the impression of the voice of fear.

C. *Articulation*

a. Method. By asynergies in articulation will be meant here the disharmonies between the action of the muscles of articulation and those of vocalization. This disharmony is regarded as one of the most important manifestations of stuttering. Since instruments attached to the lips, teeth, etc., of the subject will register the facial contortions which are characteristic of many stutterers, and thus interfere with the study of the actual processes of articulation, the writer has felt the need of an apparatus sufficiently delicate to record the sound of the vocalized consonants and at least to indicate the location on the voice records of the mutes such as the explosives, fricatives, etc., in addition to showing the pitch, intensity and, if possible, the quality of the vocal sounds.

At Stanford University and subsequently at Clark University more than a year was devoted to experiments with the Marbe smoke-ring apparatus in the hope that it could by some improvements be made to serve our purposes.[8] The attempt to use this apparatus was abandoned for the following reasons: *1.* It can only register pitch,—the amount of deposit on the paper is not a reliable measure of intensity; *2.* the rapid motion of the kymograph was a source of distraction to the subjects and made lengthy records difficult to procure; *3.* consonantal sounds cannot be recorded by this means. The flame is inclined to flare out from the drum especially when explosive consonants are pronounced; an important point in the study of the stutterer's voice is thereby lost.[9]

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[8] This apparatus consists of an arrangement by which an acetylene flame, set in motion by the vibrations of the air-waves against a current of gas that passes by a rubber diaphragm in a manner similar to that seen in the Koenig-flame apparatus plays upon the drum of a kymograph which receives deposits of soot (smoke-rings) according to the pitch of the sound.

[9] An attempt was made to obviate some of these difficulties by the use of various sorts of diaphragms. One attempt at improvement consisted in the substitution of a telephone disc for the rubber diaphragm. A brass cap was fitted over an ordinary telephone diaphragm in such a way as to form a thin air chamber over it; and a small gas burner was attached to the center of this cap. The gas was introduced over the diaphragm at the side. It was then possible to keep the vibrating body near to and at a constant distance from the flame; and the subject could be kept at a distance from the distractions of the apparatus. Preliminary tests failed to show this to be a satisfactory instrument for the study of the asynergies of articulation. These difficulties are not insuperable, however, and it is planned to renew our attempts to overcome them by the use of instruments like Hensen's phonautograph or Hermann's phonophotograph (110; 790).

b. Results. For reasons already cited the writer cannot here offer results of graphic records similar to those given in our discussion of breathing and vocalization. The findings are therefore to be reported descriptively.

By articulation is meant the molding of vowel sounds into words or syllables by the appropriate positions of the lips, tongue, teeth, hard and soft palate, and the relative openings of the nasal and pharyngeal cavities. Stutterers (without exception so far as the writer knows) can sing. Singing brings vocalization into prominence and reduces articulation to minor importance. This fact has led authorities to conclude that the consonant and not the vowel is the source of the stutterer's difficulties, although it has led another recent writer to draw the very opposite conclusion.[10] Hunt holds that stutterers have difficulties with vowels as well as with consonants, and so does Mr. Isawa of the Board of Education of Japan, who claims to have found at least one vowel stutterer. We therefore find three distinct theories with reference to this question. One theory holds that consonants alone are the source of the stutterer's difficulty; another that the vowel alone is the source of the difficulties, while a third theory holds that both consonants and vowels are responsible for the defect.

The vowel stutterers are generally described as being unable to leave off certain vowel sounds when once they have begun to sound them. This characterization is, in the opinion of the writer, wrong for two reasons: 1. The essence of stuttering consists in an inhibition of utterance, and not in an inability to cease utterance (See p. 205); and 2, what seems to be an inability to cease sounding a particular vowel is in reality an inability to combine that vowel with the succeeding consonantal sound to which the stutterer has been looking forward. This claim is in harmony with the well-known law mentioned by Bonnet (15; 52) that when a function demands the united harmony of a number of acts, if one of these acts cannot be executed (is inhibited), the others will continue to reproduce themselves until the co-ordination of the movements can finally be secured. This is a familiar occurrence with the stutterer; it is seen most frequently in the repetition of the initial syllable or letter of a word.

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[10] Bluemel says (151; v. 1; 184-185): "The stammerer (stutterer) rarely has difficulty in singing. But song differs from speech chiefly in the manner in which the vowels are produced; while there is manifestly no radical change in the consonants. The absence of the stammerer's impediment in song indicates, then, that the vowels occasion the difficulty in speech."

The writers who hold that the consonants are the sources of difficulty for the stutterer have in many cases attempted to make out lists of such consonants. A study of such lists and several attempts to secure them from stutterers have led the writer to conclude that they are not the same for all stutterers, and that they do not remain constant for the individual stutterer. Experiments have moreover shown that if the consonants that are known to be difficult for a certain stutterer are arranged so that they appear in the middle or at the end of a word, instead of at the beginning, the same difficulty is not experienced. Stutterers are also found to be able to whisper without stuttering (the writer has found only one exception to this). The writer has never seen a case of stuttering in the pronunciation of a vowel that was wholly isolated from consonants. He has tested several of his severest cases in this regard by having them read sentences by eliminating all the consonants and sounding only the vowels. If one were compelled to indicate just what point constitutes the difficulty for the stutterer it would doubtless be nearer the facts to say that the asynergy in question consists in the failure of vocalization to be co-ordinated with articulation. This would point to the syllable rather than either the consonant or the vowel. But even so it must be said that the factors that determine the difficulty in all cases seem to be not the nature of the sounds themselves but the mental experiences undergone by the stutterer in his attempts at pronouncing them. Furthermore, meaning can generally not be attached to a single vowel or consonant but it can to a syllable. This, as will be shown later (pp. 240f.), has an important bearing on the rise of stuttering.

D. Accessory Movements

Under this heading are grouped the spasmodic radiations of bodily movements which accompany stuttering but which bear no necessary relation to the normal functioning of speech. These radiations include both bodily and facial contortions, which are not merely asynergic but are surplus, unrelated, and illogical in their nature, and for that reason have been called tics.

a. Tics. Stuttering has been defined as a convulsive tic. Bonnet, however, has clearly shown the difference between the two phenomena (15). He finds (15;38) that tics are often superadded to stuttering, furthermore that stuttering and tics are of common neuropathic stock, and that one may become transformed into the other. They possess some marked differences, however. The number of persons, for instance, who are subject to tics is the same for both sexes, a condition which is not true in the case of stuttering. The most frequent age for the appearance of stuttering is from three to five, the next most frequent is from five to ten. Tics appear later, rarely coming before the fifth year. They appear most frequently from six to ten; and the next most frequent time of onset is from eleven to fifteen. There are tics of adolescence, and even of old age, whereas stuttering is thought never to begin after twenty. Stuttering may result from imitation; tics do not. Tics need no provoking cause; they appear apropos of anything or of nothing. Stuttering movements appear only during an attempt to speak. Bonnet thinks (15; 52 ff.) that stutter movements were perhaps primitively adapted to a function. Speech, it is remembered, involves not only the articulatory muscles but mimic muscles as well. In these mimic muscles we may expect compensatory reactions which have the likeness of tics but which lack their essence. The tic is a natural act, a gesture. Stuttering is a progressive attempt to speak, which is thwarted by muscular disorder, dyskinesia, and functional asynergy. In stuttering there is an attempt at motor co-ordination for the purpose of emitting a word or syllable thought of, while the tic is a useless, and involuntary movement which is complete within itself though purposeless.

Bonnet gives (15; 82) the following stigmata of ticqueurs: *1.* psychopathic predisposition, *2.* mental infantalism, *3.* feminism, *4.* mental degeneration. He believes that stutterers present neuropathic symptoms, but some of these are probably the " result of the tribulations of the life of the stutterer." Mental infantalism, if present at all, is found only during the school-age. After twelve the stutterer makes progress rather above his age. Feminism is thought to appear in young stutterers, and in certain cases also in later life, particularly in those cases that seek solitude. Over against the weaker qualities Bonnet cites cases of " illustrious stutterers " who have been " the indefatigable champions of the cause of religion, art and politics." He therefore concludes that the stigma of degeneration can not be assigned. By fixation of attention a ticqueur may control or at least decrease the violence of his spasmodic movements. The fixation of attention (See p. 238) by the stutterer decreases the power of control, and increases the violence of the movements.

Von Sarbo (122) says that verbal tics, such as the use of "you know," "you see," "you understand," etc., are especially characteristic of cured stutterers. In the writer's opinion these are not true tics but are expedients which have been purposely adopted, and which are employed to release the initial tension. The stutterer can utter them, apparently for the reason that they mean nothing (See pp. 240f.). It is of interest to know that, whereas all hysterical movements respond to mental influence, tics do not, but are even refractory to hypnosis (122; 206).

The term "spasm" is frequently applied to stutter movements. Pitres (15; 47), however, calls attention to the fact that spasms are slow, progressive, and rhythmic contractions, whereas stutter movements are quick, jerky, arhythmic, and explosive.

b. Larynx Movements. Another class of movements, believed by certain investigators to be closely related to stuttering, includes the larynx movements. The writer holds that it is incorrect to assume that because the larynx is a speech organ therefore all its movements, whether normal or abnormal, are essentially related to the function of speech. Whatever superfluous larynx movements are found to characterize the stutterer's speech are, it is held, to be regarded as being as foreign to speech as if they occurred in a remote peripheral organ, as for example, in the foot, where they do often occur, and should be set down as accessory. Gutzmann has given an account of studies made of larynx movements in abnormal speech (67). The methods employed were observation and palpation. The tip of the nose and the ear lobes were held in line and the movements of the larynx were observed while the subject talked or sang. Dermographic marks and Roentgen rays were also employed in obtaining records. Laryngographs were devised by Pilton and improved by Rousselot. Gutzmann himself used the Zwaardemaker apparatus with the Krueger-Wirth *Kehltonschreiber* for indicating the point at which the vocal sound began.

Gutzmann distinguishes the abnormal movements which begin at the larynx from those which begin elsewhere and extend to the larynx. He believes that if the cramp which closes the glottis comes when the larynx is at its lowest point there is a sudden rise from top to bottom of its position due to the crowding of the expiration air against the closed glottis. Accompanying this condition the abdominal muscles are tensely contracted, and the neck veins are distended, though the neck muscles do not seem to be contracted. In other cases various larynx and neck muscles are involved. Although

Sikorski (Ibid) claims that the cramped contraction of the neck muscles seldom changes the position of the larynx in any appreciable degree, yet he considers it important to determine which neck muscles participate in these cramps.

Gutzmann concludes his own investigations (67) by saying that the symptomatology of stuttering is almost unlimited and that general conclusions are therefore impossible. This conclusion is in harmony with the findings of the present study on breathing (See p. 229).

c. Miscellaneous Movements. Besides the classified accessory movements already enumerated there are certain ill-defined and miscellaneous movements which usually occur in the stutterer's attempt to speak. The writer has observed acts of stamping the feet, of snapping the fingers, of twisting the head, and of opening the mouth; jerky contortions of the face, whistling, pressing the tongue against the roof of the mouth, pressing the lips against the teeth, and pressing the lips together.

IV. Psychophysical

Under this heading will be noted certain physiological changes which are closely associated with, if not conditioned by, changes of mental states (138; 484); although not essentially related to the function of speech, these physiological changes are found to accompany the stutterer's attempt to talk. These are changes of heart-rate, volumetric changes of the blood supply, and galvanic changes.

a. Method. In the study of heart-rate, and of distribution of blood supply, the Lehmann plethysmograph was introduced, in addition to the pneumograph and microphone, already reported. The subject was seated as in the other investigations, and with his right hand inserted in the plethysmograph. The stylus of the plethysmograph tambour was adjusted to a position in vertical line with the others, in order to secure simultaneously *1.* the thoracic breathing curve, *2.* the abdominal breathing curve, and the records *3.* of the heart-rate, *4.* of the volumetric changes and *5.* of the voice.

The galvanic records were taken separately from all others. The Deprey d'Arsonval galvanometer was employed. The kind of electrode that proved most suited to our purposes was a large glass jar filled with a solution of sodium chloride. Large copper sheets were placed around the inside of the jar, and to these were soldered the copper wires which led through the batteries and the galvanometer. In the bottom of this jar was fixed a wooden handle which was firmly clasped by the subject during the experiment in order to avoid variations in the depth of submergence of the hand. The jars were placed on either side of a comfortable armchair in which the subject sat. An adjustable reading-rack was placed in front of him so as to obviate the necessity of movement. The method of registration was an adaptation of the one used by Radecki (116), in which light from the galvanometer mirror was thrown on a graduated scale, which was set at a distance of one meter from the galvanometer. A pointer, suspended from a cord, ran along the scale and could be made to follow the light as it changed its position. The cord which carried the pointer also carried a wire marker, which traced lines horizontally on the smoked paper of a kymograph. If now the deflections of light caused by the turning of the galvanometer mirror were accurately followed, the marks on the kymograph would indicate the exact amount of deflection.

b. Results. **a.** Volumetric changes. These results admit of general description rather than tabulated statement. *1.* It was observed that there was a marked attention drop where the subject was told to speak or read. The drop was much more decided when the subject was called upon to read, than when he was called upon to speak. This drop occurred in 73 per cent. of the cases. *2.* Immediately after the attention drop there begins a rise which usually lasts until the end of the period during which the subject is stuttering. This is found in 90 per cent. of the records. In some cases the rise is extreme, amounting in one case to 4.3 cm. in 43 seconds, and in another to 5 cm. in 31 seconds. *3.* These rises are usually not uniform; they are characterized by many fluctuations, but they have a general upward tendency. In 62 per cent. of the records the general rise is interrupted by irregularities.[11] *4.* Tests were made in which the subjects were asked to imagine themselves in situations in which they would be likely to stutter, and to recall, as vividly as possible, occasions on which they had stuttered. Some of the subjects were proficient in this, as their introspective reports afterwards showed; but the curves showed no positive changes. In certain cases there was a slight decline. 5. The most important result accruing from this part of the study is the fact that both the amount of the general rise, and the distortions of the plethysmograms are correlated with the degree of the severity of the stuttering (approximately estimated). This fact has been observed by several means. *a.* Records were made of normal individuals while reading; *b.* records were made of stutterers while speaking normally, as for instance, while some one was reading in unison with them; *c.* records were made of cases who could speak better while reading than while talking spontaneously; *d.* finally cases of severe stuttering were compared with less severe cases.

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[11] The extreme disturbances of breathing, and the various bodily movements which accompany the speech of the stutterer constitute a source of error in the use of the plethysmograph which must constantly and carefully be guarded against. The following phenomena were observed: Sudden bodily movements, especially of the hand, were found to distort the pulse curve and were thus recognized; true plethysmograms are found to be not in phase with the corresponding breathing-curves; the plethysmograms that are affected by breathing disturbances do not show these effects until from 3 to 5 seconds after the disturbance.

β. Changes in heart-rate. In tabulating the results of our study of the pulse-rate, three points of measurement were chosen; first, a point in the period just prior to the speaking interval; secondly, a point at the beginning of the speaking interval; thirdly, a point at the close of the speaking interval. The measurements were made at ten-second intervals; and the numbers thus obtained were multiplied by six to give the pulse-rate per minute.

When these results are averaged it is found that the pulse-rate for the first period is 90.2; for the second period, 99.8; and for the third period, 98.6. The range of variation of the first period is from 72 to 120; of the second period it is from 78 to 126; of the third period it is from 72 to 129. One of the subjects, *W. N.*, was only twelve years and seven months old. Lest there might be an error due to his naturally higher heart-rate, the results were again averaged with his figures left out. These averages are as follows: for the first period, 88.9; for the second period, 99.1; and for the third period, 97.2. These investigations continued through more than a year, so that the excitement due to the novelty of laboratory conditions cannot be assigned as an explanation of the findings. Preliminary tests, moreover, were carefully excluded.

From the above results it appears, 1. that the average pulse-rate is above normal in the period anticipatory of speaking; 2. that the second period is still more above the normal; 3. that the third period shows a greater average than the first, but less than the second; 4. that in general, under those conditions that are calculated to produce stuttering, the speech of the stutterer is attended by an abnormal acceleration of the pulse-rate.

γ. *Galvanic Changes.* The experiments here reported were continued through a period of only three months. While they have not gone far beyond the preliminary stage, the results seemed at least to merit mention.

During the progress of these experiments both observation and introspective evidence indicated that a state of excitement was present in the subject when he took his seat in the chair in anticipation of the painful experience of speaking. The galvanometer deflection confirmed this indication. It was therefore found necessary to devise a means of reducing this state of tension until the galvanometer reached a stable point which might be regarded as a zero point. We attempted by various methods, such as the phonograph, free and pleasant conversation, etc., to reduce the subject's tension. During these preliminaries the experimenter was in a position to observe the changes in the galvanometer deflection without the knowledge of the subject.[13] During this preliminary period of relaxation the deflection tended consistently in one direction (See Plates; see also Table V. p. 228). When the signal to read was given the deflection began to take the opposite direction, in some caess immediately, in other cases after a few seconds. Succeeding most of the speaking records there is again a reversal in the direction of the deflection and a return to the initial direction of the relaxation period. In the midst of one reading period, the subject became suddenly amused and laughed at his own efforts at speaking. This caused a deflection of 15 millimeters in a direction corresponding to that of the initial relaxation period. Some records show continuous deflection in one direction, so that the greatest deflection appears at the end of the record. Other records tend to rise to the extreme position at the central part of the record, and then gradually subside toward the end. Others show deflections that are so sudden that it was impossible to follow accurately with the tracer all the changes that were indicated by the galvanometer mirror. It may be said that the subject in whom this condition was found to exist can read well under favorable conditions. He is a declaimer in the high school. Only occasionally does he halt at a word which he has seen ahead of him. It was at such places of anticipated trouble that the galvanometer changes were obtained. The tabulated statement of the extent of these changes can be seen by reference to Table V, below.

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[13] Record 9, Table V (p. 228) shows the effect of failure to reduce this state of excitement. The subject's introspections reported excitement at the beginning; and the slight variation as compared with other records is evident.

TABLE V

The measurements given in this table are in millimeters; the time of actual changes is recorded in seconds. The measurements are made in three periods as follows: Period I is the preliminary relaxation period; Period II is the interval during which the subject is speaking; while Period III is the final relaxation period after the speaking has ceased. The extent of the maximum variation is found at various points in the speaking period. It is found by measuring the lowest and the highest points in any single, continuous variation that seems extreme. The columns under *a* indicate the extent of the variation during the periods indicated above them; those under *b* show the time during which the variation took place.

No. of Record	*Subject*	*Period* I.		*Period* II.		*Period* III.		*Maximum*	
		a	*b*	*a*	*b*	*a*	*b*	*a*	*b*
1...........	*A. N*...	25	1	47	7	...	...	...	...
2...........	*H. A*...	33	1.5	28	3.5	30	1.5	29	1.5
3...........	*H. A*...	...	...	23	2	34	2.1	...	...
4...........	*A. N*...	3	1	11	5	7	1.2	64	1.5
5...........	*A. N*...	15	1	58	3	...	...	...	...
6...........	*A. N*...	...	...	23	3.2	...	...	...	...
7...........	*A. N*...	58	4.5	65	4.6	...	...	80	1.5
8...........	*R. T*...	...	...	53	3.6	60	3.8	...	...
9...........	*A. N*...	12	1	11	5	13	0.8	18	4
10...........	*D. D*...	1	1.5	21	4.8	...	...	31	3.2
11...........	*H. A*...	...	...	3	2.5	53	3.6	4	0.7

The findings so far as the present study has gone seem to show *a.* that the stutterer is subject to a period of galvanic disturbance prior to the speaking interval; *b.* that this condition continues in varying degree throughout the interval of stuttering; *c.* that, as with the volumetric changes (See p. 226), the degree of galvanic deflections varies approximately with the severity of the stuttering.[14]

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[14] Attention should be called to record eleven in Table V, in which the stutterer is reading in unison with the experimenter without stuttering. The table shows much slighter variation in this record than in others of the same subject. In record 4 the galvanometer took the direction of relaxation while two stutterers are reading together;

V. Interpretation

It was thought best at the outset to approach the study of stuttering from the physiological side. This was done not without the expectation that these features would on examination warrant generalizations which would possess diagnostic value. The findings from the purely physiological side have, however, forced the conclusion that, taken alone, they do not represent the essential features of the phenomenon. The physiological manifestations of stuttering may be described by such general terms as "incöordinations," "disturbances," "asynergies," etc.; but the description of the essential, uniform or characteristic features has not been possible. The literature of the subject, and the writer's own studies have failed to show precisely what are those physiological manifestations of stuttering that may be called pathognomonic. The physiological accompaniments, as the foregoing results have gone to show, have not been uniform; and they have been found to be sometimes present and sometimes absent in the same individual. Nor has it been possible to establish the existence of a neuropathic diathesis, since it is impossible to know how many neuropathic symptoms are primary or causal, and how many are secondary or sequential.

On the other hand, however, it has been found that the presence of certain states of mind is a constant attendant if not an antecedent of stuttering. The writer's opinion is that these are antecedent and causal; and the indefinite character and almost unlimited variety of the physiological manifestations of stuttering go to show that the latter are symptomatic of conditions which are central and not peripheral. The possibilities of inhibitions, which may be regarded as causing the initial break in the synergic functioning of speech, are enhanced as the source of control approaches the cortical regions on account of the multiplication of the synapses, which Sherrington regards as the seat of inhibition (130; 192). The remaining portion of this paper will, therefore, be devoted to an attempt to set forth as far as possible those states of mind which are peculiar to the stutterer and which are associated with the function of speech. Since our subjects were not trained psychologists, it was naturally impossible for them to couch their descriptions in technical terminology. No attempt was made to suggest psychological terms

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curiously enough, neither one stuttered here, nor had any fear of stuttering. One of the subjects (*A.N.*) had just previously given a record in which the deflection was 64 mm. in the opposite direction.

to the subjects; and for fear of doing violence to the facts we prefer to present the reports of introspections in their original form.

VI. PSYCHOLOGICAL

A. Emotions

a. Historical. Hornig (77; 188) holds that, although fear and anxiety aggravate stuttering, they do not cause it, but are merely secondary manifestations. He grants that emotions can and do arise as effects of stuttering; and these same emotions, forming memory complexes which associate themselves with the function of speaking, arise again when speaking is necessary to hinder it by producing the inhibitions, asynergies, etc., already described. So that certain emotions and stuttering are seen to operate in a vicious circle. Wundt holds (146; 65) that speech originated in the expression of emotion, and that movements of expression, of which speech is one, have continued to be closely allied with the emotions.

Gutzmann's position on this question seems difficult to make consistent. He believes that speech belongs to the movements of expression, and that it always exhibits a pleasant or an unpleasant affective tone (48; 385 ff.). Particularly does he think (*Ibid;* Cf. p. 207, above) that breathing and voice, no less than the mimic muscles, are responsive to emotional conditions. Emotions, he says, always change the rhythm and the depth of breathing, the costal breathing being more responsive than the abdominal. The fact that costal breathing is more disturbed in stuttering than abdominal, indicates that emotions play a part here. He holds that the emotions affect the action of the vocal cords; and that they play a prominent part in the speech development of the child. The extreme emotion felt by the deaf-mute child on first learning to utter a word is cited in confirmation of this view. (*Ibid.,* p. 395.) He grants further that the cortical and infracortical regions are more ready to discharge a function when stimulated by emotions; yet he holds that emotions are of no fundamental significance in the causation of stuttering. He believes that the child experiences no intensive depression on account of his stuttering, but that this feeling of depression comes only with maturer years and increase of intelligence. Many stutterers, he says, have no experience that could be called "anxiety before speaking," though in reading, such an experience may appear in certain cases. He claims that he had observed "visual indications" of emotions in one subject just before coming to the letter *k,* which usually caused difficulty; but he asserts that the very contrary conditions are also found. His position is (66; 203) that it is possible to eliminate emotions from stuttering without altering the defect.[15]

Meumann (107; 578) holds that every child stammers at some time during his first or second year. When stammering has occurred in the child, the ridicule and mocking of his associates may produce shyness and depression, which are fertile soil for the production of word-anxiety and lack of confidence,—both of which are symptoms which characterize the stutterer. One seems to be justified in supposing that stuttering never arises in a permanent form except through the intervention of an emotional shock. Mispronunciation, ataxia, lapsus linguae, and Versprechen may occur under certain circumstances in normal individuals; and these are similar to the first stage of stuttering. They are not pathological; but in the young they may readily become so. An American psychologist has given the writer a valuable illustration of this in a personal experience, which is quoted *verbatim:* " Between the ages of sixteen and eighteen I underwent a dental operation for straightening the teeth. During the time when I was wearing a plate with attachments, my speech was seriously interfered with, and there were many sounds that I was unable to articulate clearly. On an occasion when a number of young people of my own age were present, I attempted to say something about preliminary college examinations; the word " preliminary " caused me great difficulty, and finally, after several attempts, I was obliged to give it up and was greatly embarrassed by the incident. After the removal of the plate I still found myself unable to pronounce the word (preliminary) clearly and met with another embarrassing experience in consequence. I have never since felt safe in attempting to use the word in conversation, for although there is absolutely no physical defect and although I can always pronounce the word without difficulty by pausing a little and making an effort, yet any attempt to use it in conversation is likely to be followed by disaster. In consequence of this I invariably avoid using the word and substitute some other expression." The genesis of stuttering through imitation may, in the writer's opinion, be accounted for on the same principle. Prior to the emotional experience which changes its character and makes it pathological, the imitative stuttering develops an increasingly habitual character. Bonnet reports (15; 55) that in the case of some stutterers the mouth is dry and bitter, and the tongue feels as though it were covered with a plaster-like substance. He finds that some stutterers have veritable trances during which one may observe all the secretory and vaso-motor phenomena that characterize emotions. He has noticed (15; 76) that stutterers have less difficulty in speaking when partially intoxicated, because, as they report, they do not feel the same dread under these conditions. He agrees with Merge and Feindall that stuttering is more a matter of the emotions than of the intellect or the will; and with Morel that stuttering can, like obsession, be defined as being at bottom a morbid state of the emotions which expresses itself by motor difficulties, particularly in articulation, arhythmic breathing and vaso-motor disturbances. Stutterers on going to purchase a railroad ticket or an article in a store will often take along a card on which is written the name of their destination or the article wanted. Bonnet (15; 71) regards this as an unmistakable sign of word-fear, which he thinks arises from the age of 8 to 9, and is particularly common between 15 and 25.

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[15] It seems hardly necessary to remark that Gutzmann's " visual indications " are as inadequate for the detection of physiological manifestations of emotions as were the old methods of observation and palpation for the detection of abnormalities of breathing. The writer takes issue with Gutzmann's position and presents data secured both from physiological registrations, and from introspective reports.

If Ribot is correct in holding (118; 171) that, in certain individuals, emotional memories are possible, while in others they are not, it would seem certain that stutterers belong to the latter class. Titchener claims that two classes of individuals are not to be differentiated in respect to this capacity; but that emotional memories are possible in every individual if the experiences have been sufficiently pronounced. "When a boy is flogged at school," he says (138; 493 ff.), "he has, besides the immediate pain of the flogging, all sorts of anticipatory and subsequent stirs of organic sensation,—flutterings, sinkings, chokings, breath-catchings, nauseas. If, when he recalls the flogging in later life, the cortical excitations that underlie his memory-ideas revive the splanchnic and other excitations that constitute the stimuli to organic sensations, then the scene comes back to him with its affective coloring upon it." The stutterer apparently undergoes most, if not all, of the organic sensations enumerated by Titchener. And, upon recall, his painful experiences with attempts at speaking not only bring back memory-ideas with "affective coloring", but they revive in varying degrees the original organic experiences.

Appelt (1; 15) thinks that stuttering "is purely a psychic ailment at whose core stands the dread of speaking." This acts as "an inhibiting agent" to obstruct the speech.[16]

b. Results. Our data have been secured by two methods. *1.* The method of expression (138; 240 ff) employs apparatus for the registration of bodily changes during the act of speech by the stutterer. Detailed reports have already been made of the studies of breathing, vaso-motor, pulse, and galvanic changes. They therefore need only to be counted in this connection as objective data in the study. *2.* The method of impression has, in the present research, been a difficult one to employ. The difficulty has been due not only to lack of psychological training on the part of the subjects, but to the fact that even on immediate retrospect the stutterer's memory seems vague and hazy as to details. He is often wholly unable to recall what he has read only an instant before. Indistinct memories of his environment and of the bodily sensations connected with his strained efforts at speaking, especially his remembrance of sensations in the throat, the chest, and the abdomen constitute his memory content. In some cases a feeling of complete bodily relaxation quickly supervenes to affect the recall. Two subjects reported that this relaxation was so pronounced that they felt as if they would like to lie down and sleep after a siege of stuttering. One subject reported that while talking his attention was divided between what he wanted to say and what were his chances of saying it without stuttering.

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[16] The part played by the emotions in stuttering has an important analogy in certain hysterical phobias. To Stekel it is more than an analogy, for he contends (136; 300) that stuttering is a form of hysteria. The analogy is shown in certain pathological cases, for whose description the writer is indebted to Dr. Edward Cowles of Clark University.

The first is a case of the fear of sweating reported from the McLean Hospital, Waverley, Mass. This subject was frequently distressed when at church or social gatherings by feeling very warm and by excessive perspiration. The hospital report states that "the patient might be playing cards when suddenly he would think that he was going to get hot; he would not sweat for a time perhaps, but the idea that he was going to sweat remained in his mind; he tried to get rid of the thought of sweating; he would become worked up and stirred up, feel a thumping about the heart, a disagreeable sensation in the region of the stomach, and the perspiration would come out."

The second case is that of the fear of nose-bleeding, which is reported in the *Russki Medizinski Vestnik,* December, 1901. A young man became subject to nose-bleeding, which was generally preceded by vaso-motor disturbances like blushing, blanching, chill and general malaise. If he happened to leave his handkerchief at home, or if he found himself in a place where it would be particularly distressing for his nose to bleed, it would then be most likely to occur. If while attending a lecture at the university the thought of nose-bleeding occurred to him it would bleed. (One can ask, in this connection, whether the fear or the bleeding constituted the obsession. And the same question is also pertinent in the case of stuttering. The writer believes that the weight of evidence, in the present study, shows that the true obsession is mental in the case of stuttering.)

All of our subjects reported that certain letters and collocations of sound looked ominous to them, if they ran their eyes ahead while reading. Synonyms are constantly employed as substitutes for the difficult words.

It was found that if some one read the selection aloud with the stutterer, he was always able to read without stuttering. An attempt was made to obtain data regarding the states of mind under the two situations, in which the stutterer is reading alone, and in which he is reading in unison with some one else. (Graphic records have already been presented, showing that there are differences in physiological processes in the two cases, p. 228, footnote, etc.).

Many subjects were sure that their feelings were different in the two cases, but were unable to describe the differences. One subject reported that he had "much less fear of failing when reading with some one than when reading alone." When reading with this subject (*A. N.*) the experimenter frequently skipped words, and even whole sentences; yet it was found that the stutterer continued to read through these intervals without stuttering. His introspective reports testified that he felt "secure" while reading alone in these intervals because he knew that if he "got stuck" he would be aided and prompted by the experimenter.

Other subjects reported "fear," "anxiety," "uneasiness," "distrust" of themselves, "lack of confidence," "dread," etc. "A feeling of strain" was frequently reported. One subject said that he often felt himself sweating; and many felt the rapid beating of their hearts.

In describing his mental state during stuttering one subject (*H. A.*) said: "When I am about to speak, I am usually in a somewhat excited state, sometimes more so than at others; when I get into that state I am unable to control myself and I fear the outcome. I experience an internal trembling and I frequently wonder if I show my excitement to any great extent. I wonder sometimes as to the effects of this on my physical condition. Often I wish that I would somehow vanish when I am in this condition; and at other times I should like to show my ability in other lines, such as starting a fight or throwing some object through the window or door; it seems as if that would relieve my state of nervousness."

H. A. was found to be able to read fluently a card with whose contents he was familiar, especially if he had read the card before several times. On being asked to reproduce the contents of the card he would stutter severely. When asked to describe his feelings in these two cases he reported that in reading the card he felt "confident" that he could read it as he had done before; that he felt "passive," or "indifferent." When asked to reproduce the contents, he reported that his feelings were changed, that he now felt a "lack of assurance," that he felt himself "getting excited." He would try to prevent this but the effort to do so made him worse. (Cf. the case of phobia, p. 233, footnote). On being asked to give the contents of the card he felt more self-conscious than in reading a familiar selection; he reports that his attention is "very poor" while talking, and that his muscles are very tense, especially those about the throat. Several subjects reported a "feeling of dread" before approaching a stranger to whom they knew they must speak. One subject (*A. N.*) said that if he entered a store to make a purchase and found no one there whom he knew, a dread seemed to take hold of him, and often he made his escape without purchasing.

The introspections also showed that after the speaking interval there continues for some seconds a state of excitement which gradually subsides to an indifference-point. This is corroborative of the physiological findings already mentioned (p. 228, and Table V.).

B. Attitudes

In addition to the more pronounced types of mental disturbances which have been studied by the foregoing methods, there is to be mentioned a group of mental states which it seems best to designate as "attitudes." These states are not so pronounced or intensive as affections or emotions. They do not so fully emerge into consciousness, and hence they do not so readily admit of study; but they nevertheless exercise an important influence on stuttering. Binet (13) holds that in the attitudes, whether consciously perceived or not, we have the physiological basis of the emotions. Ferrari (36), in agreement with this position, suggests that the autonomic nervous system is the anatomical substrate of the emotional life in general as well as the attitudes mentioned by Binet.

The assumption of the existence of such attitudes, which differ from emotions perhaps only in degree, seems necessary to account for the extreme physiological effects which sometimes attend what appear to be slight environmental changes. By means of this hypothesis it also seems easier to account for the strength of the morbid associations which characterize stutterers. These attitudes arising from apparently trifling causes are again and again found to determine the stutterer's ability to speak.

It is interesting to note that Geissler's study of consciousness under negative instruction holds that suppression or inhibition of articulation may be either ideational or attitudinal (40). Scripture (129) believes that stuttering is a psychoneurosis, whose essential pathology lies in the disturbance of the stutterer's feeling toward others. This disturbance of the feeling toward one's environment is the phenomenon which is here to be discussed as an attitude. It appears in all degrees from what may be regarded as a primarily intellectual *Einstellung* to the stage at which it passes into an emotion, such as fear.

In consequence of our lack of subjects trained in introspection, no attempt could be made to analyze these states of mind; hence the data herein submitted are necessarily objective and sometimes merely inferential. Sufficient facts are at hand, however, to make the point clear. The number of these facts could have been extended indefinitely. Appelt (1; 101), who was himself a stutterer for many years, says that traveling in a strange country has a marked salutary effect on stuttering; pleasant company has the same effect. He finds further, a fact often noted, that in the presence of those who are in authority over or in any way superior to the stutterer, his difficulty is more pronounced. This is particularly true if his auditors watch him closely.

It is usually easy for stutterers to talk to children or to animals. The thought that his observer is unacquainted with his difficulty is also helpful to the stutterer (1; 102). Nearly all stutterers are able to speak or read without stuttering when alone. Many stutter in conversation but do not stutter in public speaking. The most helpless stutterer studied by the writer had during one summer a position as "spieler" for a side show at a summer resort. This position required him to stand in front of the tent and announce to the passers-by what was to be seen within. He filled this position with success. He stuttered violently in conversation; yet, whenever he was called upon in the laboratory to give his side-show "spiel," he could do so without stuttering. Such expedients as changing their style of speaking, as imitating some one's peculiar form of speech, as getting away from their accustomed personality and playing a rôle usually make it possible for the severest stutterer to speak fluently and without difficulty. Pitres (15; 102) tells of a clown who played his rôle perfectly, but who stuttered so badly in private conversation that he could not even say "good morning." Many stuttering actors show no stuttering when on the stage. Masks are found in many cases to relieve stuttering. One of our subjects (*A. N.*) reports that if he can get into the spirit of a poem as he reads it aloud, and can feel its meaning he can read it without stuttering.

Bonnet (15; 77) gives an instance of the use of smoked glasses in certain cases for the purpose of giving a feeling of isolation, which was helpful to the subject. Two of the writer's subjects reported that they had less difficulty when speaking in the presence of distracting noises. One subject had a position in an office near a railroad. He found that he could answer the telephone when a train was passing, but otherwise he could not. One subject reported that he could talk better in a store if the counter over which he had to speak was a narrow one than he could if it was a wide one. Stutterers are delicately responsive to the moods, tempers and attitudes of their associates. They seem to "feel the presence" of others quickly and profoundly; and their ability to talk is profoundly influenced by this factor.

C. Imagery

It is a well-known phenomenon in psychology that the kinaesthetic images of a certain movement of the body affects the power of voluntary control of that movement (3); and this phenomenon is confirmed by the familiar facts of stuttering, which seems to take place more readily in those organs whose functioning is too complex to admit of direct representation in consciousness. The act of breathing, for instance, can be imaged only partially and more or less abstractly, because the working of the diaphragm can not be imaged and can be controlled only through the lower arch of the abdominal wall. The diaphragm is especially concerned in the breathing abnormalities found in stuttering (66). To co-ordinate vocalization with breathing introduces the function of another complication of muscles which does not admit of direct imaginal representation. The action of the articulatory organs does admit of representation in consciousness, and although stuttering speech manifests itself to the eye and to the ear as being primarily a disturbance of articulation, the real asynergy has been shown (p. 218, above) to be not merely in the articulatory system, but between this system and others, especially that of vocalization. Whispering, which involves only the musculatures of breathing and articulation, is usually found to be possible for stutterers.

The organs of speech are neither completely under voluntary control, nor are they exclusively non-voluntary. The abstract act of speech may be called voluntary; but the coordinated action of the muscles which execute the details of speech is non-voluntary. Speech movements, says Hoepfner (76; 268), are the first to lose their motor (concrete?) character and to become what must be called abstract (begriffliche) movements. The conscious antecedents of speech movements seem to be general, vague, and except in the articulatory movements, lacking in details. More and more as speech matures is the attention shifted from the physiological process to the content of the thought to be uttered. It is suggested (32; 263) that the cortical centers send their impulses to groups of muscles and not to individual muscles. Stuttering, it is to be noted, is not confined to speech. Other groups of muscles which are comparable to the speech muscles in the degree of complexity and in the abstract character of the conscious antecedents of their functioning show this same phenomenon. The writer has had one case of stuttering in deglutition co-existing with stuttering in speech. Bonnet (15; 328) reports a case of stuttering connected with defaecation and deglutition. Scripture (128) and Neumann (112) report cases of stuttering in writing.

D. Attention

Zoneff and Meumann (149) have shown the effect of attention on the quality of the breathing-curve. They find that in concentrated attention and in the emotions, the thoracic curve is more affected than the abdominal. (*Cf.* the findings reported on p. 215 of this paper.) It has long been recognized that attention plays a part in stuttering. It does not seem, however, to exert its influence only by direct action on the breathing-curve, but rather in more indirect fashion by affecting the morbid associations which themselves are the more direct antecedents of stuttering.

The surgical operations practiced by Dieffenbach (28) are now thought to have owed whatever ameliorative effect they may be credited with to their influence on the attention of the subject, or to suggestion. Placing corks or wedges between the teeth, shrugging the shoulders, tapping with the feet, pinching with the fingers, whistling or counting before speaking and numerous similar therapeutic expedients, all of which have been known to be effective in certain cases, seem to owe their efficacy to the fact that they distract the attention of the stutterer from his difficulty, and that, in consequence, they afford him a relief from the morbid inhibitions by which his speech is hindered.

Stuttering has frequently been alleviated by the act of writing during speech. Many stutterers can speak perfectly while sewing, embroidering, or playing the piano. Others resort to blowing the nose, to scratching the head, or to stroking the mustache before attempting to speak. One subject (*A. N.*) reported that in reading aloud alone, which he could do without stuttering, he often stopped to wonder at his success; and whenever he did so, he at once began to stutter. If alone, he could recover himself; but when any one else was present, he could not. One person of normal speech, a psychologist, reported that there was one word which he had difficulty in using in public speaking. If he used the word without thinking about it, he had no trouble in pronouncing it; but if his attention should fall on it before he got to it he found it prudent to avoid the use of it. This, in intensified form, is the common experience of the stutterer. It has already been noted (p. 221, above) that the so-called difficult consonants are generally found at the beginning of words. If the stutterer is able to begin a word he is in most cases able to complete it. It is to be noted that in all reading, attention is most keenly concentrated on the first parts of the words. The introduction of rhythm into speech has often had a marked effect on the stutterer's ability to speak. Rhythm, expression, and distraction from fixed ideas constitute the essential principles of treatment according to Scripture (125).

Kussmaul (85; 23) stated that stuttering did not exist among the Chinese because of the rhythmic character of their speech. (The 1910 edition of his book, edited by Gutzmann, says that this defect should not exist among the Chinese.) The writer has made sufficient investigation of this topic to justify him in saying that stuttering does exist among the Chinese. Besides numerous private inquiries among persons who have lived in China and among those who have lived near Chinese settlements in America, an attempt was made to ascertain if the Chinese government had taken notice officially of the existence of the defect. The government had not taken official action on the question; but Mr. Jung Kwai, Secretary of the Imperial Legation at Washington, D. C., in answer to an inquiry, says that stuttering is by no means unknown in China, and that more than one member of the Legation at Washington is subject to this defect.

Stuttering cannot, therefore, be said to be essentially related to rhythm in speech. The beneficial results derived from introducing rhythm into speech seem to ensue only so long as the innovation is a novelty to the stutterer, and therefore capable of affecting his attention; its therapeutic value is lost when it is a natural characteristic of speech as is the case in the Chinese language.

It is to be noted in conclusion that the wide variety of therapeutic measures employed for the treatment of stuttering could have ameliorative effects only through their influence on some common factor such as attention.

E. Responsibility for Aufgabe

Associated with the emotions and attitudes already mentioned, the realization of his responsibility for speaking weighs heavily on the stutterer. When a question is put to him, for instance, he feels the necessity to speak. If he does not speak he realizes that unpleasant consequences will follow; the persons with whom he has attempted to speak will become amused, or embarrassed, if not angry. If he were dumb or deaf the situation could be made intelligible to his auditors, but this cannot be done in his case. If he begins to speak and halts, unable to go further, the responsibility to proceed and the heightened feeling of his inability to do so overwhelms him. The realization of that demand, the idea that something is expected of him by way of reaction or reply or communication in consecutive speech, that compulsion arising from a question put directly to him, or a social or business situation in which he may find himself, is the state of mind here meant. If it were necessary to choose some single psychological term in present use to designate this state, that term would be the "*Aufgabe* consciousness." With the stutterer the realization of the *Aufgabe* is keen, if not morbid; and, as will appear from the facts, it determines his ability to speak in many instances, and is not confined merely to "coloring" his reactions. As typical of this feeling of the obligation or necessity to speak Bonnet (15; 69) mentions a stutterer who said that he possessed one good thing which he could use only when he had need of it, namely, speech. One of the writer's subjects (*A. N.*) said that he never had any trouble talking nonsense. Another subject (*H. D.*) reported that when called to the telephone he would frequently take the instrument in his hand and while raising it to his mouth would keep saying "hello" without removing the receiver from its hook. The instant he took the receiver off and got into connection with the parties at the other end of the line, and realized a necessity to speak, he became powerless to say anything. Another stutterer reported that he could talk better to his mother than to any one else because she seemed to have the ability to anticipate what he meant to say and could relieve him, at various points, of the necessity of continuing to speak. *H. A.* reports that he is helped if his auditor seems indifferent to what he is saying. *H. D.* states: "I stutter worse when a specific answer is needed, and when a person is looking at me to hear my answer; it is very hard for me to ask for a transfer, upon any particular car-line, such as Lincoln Street, Greendale, etc. Here a particular word must be spoken; there is no opportunity to substitute a word which I could pronounce more easily." Gutzmann has noted that the stutterer finds greater difficulty when a choice of words is impossible (48; 398). *A. N.* reports that he is less able to speak if he realizes that some one is watching him; the mere feeling of the presence of another person often renders him unable to speak.

All of our subjects demonstrated their ability to read without stuttering when some one read in unison with them. The writer feels justified in concluding that, under these conditions, the stutterer is not so clearly conscious of the *Aufgabe.* This relief from the complete responsibility imposed by the *Aufgabe,* together with the accompanying or consequent changes in emotion-states, already described as characteristic of these situations (pp. 231ff, above), seems to be an adequate indication of the principle involved.

In certain tests, already described (p. 225f.), the stutterer was asked to recall circumstances under which he had stuttered or would be very likely to stutter. The results were wholly negative. The reason seems to be that in this case the stutterer was entirely relieved of the task of speaking; and all efforts to revive the physiological processes by mere recall failed.

The assumption of the *Aufgabe* is only possible when speaking has meaning. Breathing, vocalization and articulation, taken separately or in co-ordination, are possible for the stutterer provided they do not have to coöperate so as to produce significant speech. That singing is possible to the stutterer seems to be due for the most part to the fact that singing consists chiefly in vocalization (p. 220, above), and to the fact that singers do not so much feel the responsibility of conveying some meaning to their auditors.

F. Psychoanalysis

An impetus to the discussion of psychoanalysis in its relation to stuttering was given by Gutzmann (55) in his review of two books on the subject (*Eine neue psychologische Behandlungsmethode des Stotterns* by Netkatschew; *Nervöse Angstzustände und ihre Behandlung* by Stekel). In his discussion, Gutzmann adheres (55; 68) to his original somatogenic theory of stuttering. He claims that the emotions present are secondary and not primary; and that stuttering is not an anxiety neurosis but an incoördination neurosis due to congenital weakness of the speech apparatus. Stekel (p. 233, footnote) holds to the psychogenic theory of stuttering, and classifies it as a form of hysteria with an anxiety mechanism which originates in sexual traumata.

Laubi had taken the position (91) that stuttering is due to a mental cause but is dependent upon peculiar diathesis in the form of congenital or acquired structural changes in the nervous system. The fact that it yields to such a variety of treatments warrants him in calling it psychogenic. In a subsequent discussion (90) he cites a case which he claims to have improved by psychoanalysis. Emotional memory-complexes he finds to be the cause of stuttering. The emotional excitability may be either inherited or acquired. In the latter case, if the emotional experience is not originally related to speech it may become so related by a process of transfer (Freudian). For instance, if stuttering should arise in a child as a result of its being bitten by a dog named Schnauz, the emotional complex will be connected with the memory of that name; and the child thereafter will probably be found to stutter on all *sch* sounds. Gutzmann replies (56) that hypnotic-psychoanalytic methods have not been successful in the treatment of stuttering, that strong mental stimulations may cause stuttering but only under two conditions,—when the excitement is strong, and when the individual is susceptible. Structural modifications are produced in both cases; and these he regards as the essential causes of the defect.

Appelt (1) holds to the Freudian view of stuttering. He attempts to show (1; 109) how erotic impressions, which arise from fondling in childhood, come under the influence of educational restraints, and may for that reason give rise to intensive inhibitions. The rise of stuttering depends upon the extent of the repression (*Verdrängung*) and the peculiar diathesis of the individual. He holds that shock, fright or disease never produce stuttering, but that these factors are often regarded as causes by parents and physicians, whereas the real causes lie farther back. Imitation can only cause stuttering where the imitator and the imitated have the same complexes. Freudian identification must be present in the form of an unconscious inference that if the stutterer observed can have such paroxysm of speech, the observer himself may also have them since he possesses the same complexes. Appelt classes stuttering (1; 133) with the phobias and the obsessions. The physical and mental symptoms of stuttering are "merely the projections of the conflicts which have accumulated in the emotional complex. All paroxysms of stammering (stuttering) are produced by energy which is supplied by suppressed libido."[17]

The writer is unable to accept the Freudian interpretation of stuttering for the following reasons:

1. He agrees with Gutzmann that the employment of the method of psychoanalysis has not been sufficiently successful in the treatment of stuttering to warrant its adoption. Treatments based upon other diagnoses have been much more successful. Mere suggestion, wholly aside from the Freudian hypothesis, seems sufficient to explain the results of the use of the method.

2. Emotional memory-complexes in children may be accounted for without reference to the sexual element from the facts, *a*, that speech in general possesses a genetically emotional character; *b*, that speech in the child is usually attended by stronger feelings and emotions than in the case of the adult; *c*, that morbid conditions easily arise with the child from various causes, such as embarrassment resulting from errors of speech, from criticism, from ridicule or from mockery; *d*, that the child at an early age is compelled to use the complex musculatures of speech without the possibility of developing slowly by the use first of the more fundamental and then the accessory, according to the law for the best development of motor co-ordination; *e*, that emotional memory-complexes may arise from nervous shocks other than sexual.

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[17] Scripture (*Stuttering and Lisping*, New York, 1912) seems at the present to accept the Freudian view of stuttering.

3. There seems to be no adequate reason for supposing that only those emotional complexes which have a sexual origin can cause stuttering. Such a theory of stuttering, when one attempts to apply it, is found to necessitate the introduction of another theory, namely, that of transfer, to explain how sexual traumata are switched into the function of speech.

4. The hysterical conditions supposed to be caused by the suppression of the sexual complexes seem to be more characteristic of females than of males, while stuttering is many times more common in males than in females. (See pp. 207f.)

G. Association

In a former section (p. 221) it was shown that the difficulty encountered by the stutterer in the pronouncing of certain letters is determined, not by the nature of the letters themselves, but by the experiences associated in the stutterer's mind with the use of those letters. One of the subjects, *W. N.*, acquired a persistent tendency to stutter in pronouncing a particular word in consequence of one unsuccessful attempt to give a number while going through his regular test in counting from one to twenty. To instruct him to be on his guard as he came to that number seemed each time to furnish a positive suggestion to stutter.

Bonnet (15; 75) cites a similar case: A soldier had answered "present" for three months without stuttering. One day when the roll was called by a sergeant who had punished him the day before, he was unable to answer "present"; and he continued to stutter only when that particular officer called the roll. Bonnet holds that any case of stuttering which originates in reading will remain a case of reading-stuttering. The same phenomenon appears in reciting, in telephoning, etc. This may be called "associative stuttering." It is not, however, to be construed as meaning a new variety of stuttering. If we extend the notion of association beyond the range of conscious ideas and make it include a wider range of physiological processes, all stuttering may be said to be associative. As a descriptive term there appears no objection to regarding stuttering as an "associative neurosis" provided one agrees with Donley (33) that this does not represent a "disease entity," or a "disease form," but only a "functional variation." The rise of vaso-motor, secretomotor, and other physiological changes through association, when there is no conscious recall, is not peculiar to stutterers alone.[18]

Langfeld's studies (87) show that in association experiments, where the reagent is instructed not to respond with the name of the presented picture, the negative *Aufgabe* may produce a "locking of the muscles of the organs of speech," which sometimes occurs without "consciousness of the instruction or of a judgment." He finds that suppression may be present in various forms, from a voluntary act to a purely automatic reaction. In the case of the stutterer, the *Aufgabe* consciousness involves no explicit negative instruction but the complex of associations which is antagonistic to the positive instructions is effective in accomplishing the inhibition.

Hoepfner (76) holds that stuttering is an associative aphasia. This term is objectionable only in that it generally implies an organic lesion or a structural malformation.

The mechanization of associations and the fixing of neural paths is unquestionably present in stuttering; and, in so far, stuttering falls within the category of habit, unless we choose to limit the notion of habit to the modifications produced by repeated performance.[19]

Stuttering arising from shock, from disease, and from imitation (which in some cases may be repeated very few times), is obviously not due to repeated performance. Just how being bitten by a dog (53; 92) can produce stuttering in the same fashion in which habits are acquired is not easy to see; but it is possible that, in such a case, speech may be regarded as the natural mode of defence through which the nervous shock found expression, and in doing so fixed the line of nervous discharge as effectively as repeated performance would. In such a case the whole anxiety neurosis or association complex must become switched off into the function of speech, so that it no longer attaches itself to the particular experience of being bitten by a dog, which was the original cause of the shock, but to the general function of speech. It would, then, no longer be a general condition of anxiety but would be connected only with the act of speaking.

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[18] Betz (11; 273 ff.) reports an incident which seems to have a bearing here: One day, on a street-car, he saw a fellow passenger light a cigar and settle back in his seat for a comfortable smoke. But a sudden jolt of the car dislodged the cigar and threw it upon the floor; and the ludicrousness of the incident caused Betz to smile. A few days later he saw a stranger whom he felt he had seen before. In his prolonged effort to "place" the stranger he found himself smiling; and the reproduced smile proved to be the medium through which the street-car incident and the smoker's face were finally recalled.

[19] Cf. Baldwin and Stout's discussion of habit in Baldwin's Dictionary.

The part played by the element of conation in stuttering also seems to differentiate it from habit as commonly understood. In the first place stuttering in all of its pathological aspects can scarcely constitute the goal of voluntary effort. The imitation of stutter movements, to be sure, may be voluntary and as such may obey the laws of the acquisition of habit, but when this mere imitation of the stutter movements gets to be actual stuttering a new condition arises in which the individual becomes a victim not merely of the motor habit acquired through a few repetitions but a victim rather of the associated mental conditions. To illustrate, the writer has the history of a case in which a child became a stutterer by attempting to show his mother how a clown had stuttered in a circus. If stuttering were a mere motor habit it is evident that the clown would have been much more of a stutterer than the child by reason of the constant repetition of stuttering in his daily performances.

Again, just as "repeated performance" is not essential to the rise of stuttering, and when carried out does not always result in stuttering, so it is found that "cures" are not always effected in the same fashion in which one motor habit, for instance, is supplanted by another. Many stutterers, for instance, are cited (125, 126, 127) who ceased to stutter almost immediately when shown how to introduce some new variation, such as rhythm, into their speech.

It is to be noted further that if stutter movements were simply motor habits like peculiarities of hand-writing, for instance, they would be expected to occur with equal if not increased facility in the absence of attention. It has already been shown (p. 239) that stuttering tends to disappear when the stutterer's attention is distracted from his speech.

From the above considerations the writer has concluded that the term "habit" does not wholly describe the true conditions of stuttering, and that if the term is to be employed it should be used to designate rather the mental states which, it is held, constitute the essential pre-conditions of stuttering. But it is to be borne in mind that these mental states do not obey the law of habit by coming into being through volitional conation, nor are they marked by a progressive decrease of this volitional element until the habit has become fixed. Instead they come involuntarily and there is no evidence against the assumption that they continue to be opposed in equal degree by the will of the stutterer.

VII. Heredity

The study of heredity in its relation to stuttering would not come within the scope of an experimental attack upon the problem, such as the writer has undertaken. It will, therefore, be mentioned only in so far as it has a bearing on the conclusions reached in the present study. The facts so far known seem only to warrant opinions rather than definite conclusions regarding the inheritance of the defect; and the opinions differ. Gutzmann believes that stuttering is due to inherited peculiarities of structure (55, 61, 122, 180); and this view is accepted by writers less well known (15;21). McReady (97) classifies stuttering with amusia, and states that it is due to "biologic variations" in the brain centers. This view illustrates the futility of undertaking to base stuttering on physical stigmata. A distinguishing characteristic of stuttering is its intermittence (p. 205); if persons afflicted with amusia were found to possess the power of tonal discrimination when in certain states of mind and not when in others there would be some grounds for classifying the two defects together.

One cannot but admit the possibility and even probability that a favorable neuropathic diathesis is present in the stutterer; but it is acknowledged that this does not explain why many persons of neuropathic diathesis never stutter. The causation of stuttering by imitation also seems out of accord with the structural explanation of the defect. To establish the hereditary character of stuttering one must find a stutterer who is descended from stuttering ancestors, and who has never observed stuttering in others, which is obviously a very difficult thing to do. One of the writer's subjects (*W. N.*) was the son of a stutterer; and the father reported that he himself had acquired the defect by imitating a school-mate. To assert that the son inherited the father's acquired character would be to cut the knot of the whole inheritance problem at once. Such errors as this, it is feared, have crept into statistical reports on the problem of heredity in stuttering.

VIII. Conclusions

1. The motor manifestations of stuttering are found to consist of asynergies in the functioning of the three musculatures of speech,—breathing, vocalization, and articulation.

2. Accompanying these asynergies there are also to be found tonic and clonic conditions of other muscles which are not involved in normal speech. These accessory movements tend to become stereotyped in each individual.

3. Stutterers are found to differ widely in type of asynergy, and particularly in accessory movements. It is, therefore, impossible to assert that any form of breathing, of articulation, or of vocalization constitutes the essence of stuttering.

4. Besides the motor manifestations of stuttering there are other accompanying conditions which consist in disturbances of pulse-rate, and of blood distribution, and in psycho-galvanic variations; these changes appear before, during, and after the speaking interval. The intensity of these manifestations is found to vary approximately with the severity of the stuttering.

5. The essential condition of the rise of stuttering seems to be a complex state of mind, which should be classified generically as feeling, in the wider sense of that term (138; 227). It is to be noted, however, that the quality rather than the intensity of these feeling states governs the rise of the defect. Certain forms of excitement, such as that incident to speaking in public, for example, caused stuttering to disappear entirely in over fifty per cent. of our cases.

6. In general the feelings that tend toward inhibition or depression, such as fear, anxiety, or dread, or shame or embarrassment, are the ones that are most likely to be the precursors of stuttering. Probably all of these attendant mental states operate in a vicious circle in that they act as both cause and effect. The writer is of the opinion that in general the permanent condition of nervousness that is thought to be characteristic of stutterers should be regarded as effect rather than cause.

7. The states of feeling that have to do with the production of stuttering vary in degree from strong emotions to mere attitudes or moods. These latter are often so slight in degree that it is difficult for the subject to report their presence, and yet by the logical "method of difference" it seems necessary to consider their presence as a causal factor.

8. In addition to states of feeling, stuttering seems to be affected by the quality of mental imagery, by attention, and by association. All movements that, like those of speech, are incapable of clear and detailed imaginal representation in consciousness are, in the same way as speech, liable to functional disorders that are analogous to stuttering. When the stutterer's attention can be distracted from his speech his stut-

tering generally ceases. The affective and emotional experiences associated with the pronunciation of sounds, rather than the nature of the sounds themselves, determine the rise of stuttering.

9. Stuttering, therefore, seems to be essentially a mental phenomenon in the sense that it is due to and dependent upon certain variations in mental state. Hence the study of stuttering becomes a specifically psychological problem; and it seems evident that a detailed analysis of all the various aspects of the phenomena of stuttering will furnish important contributions to general psychology.

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X. ILLUSTRATIVE PLATES

Typical records are here reproduced to illustrate certain physiological phenomena that accompany stuttering, not all of which are amenable to detailed description or tabulation. The records read from left to right; the time is recorded in seconds. In all the breathing records the amount of inhalation is registered by the downward movement of the stylus, that of exhalation is indicated by the upward movement; the duration of both inhalation and exhalation is measured by the extent of the movement in the horizontal direction. By comparing all points of each curve that lie on a line drawn perpendicular to the line of movement of the drum the temporal relations of the different phases of the processes recorded can be determined.

PLATE A gives two records taken at different intervals and showing the thoracic curve above and the abdominal curve below in each case. Below the abdominal curve of each record, the registration of the microphone is shown by the up and down movements of the stylus. The abnormality of this record is shown in the arhythmic character of the breathing curve and also in the scantiness of the voice record.

PLATE B is reproduced for the purpose of comparing it with record C following to illustrate the possibility of both normal and abnormal speaking in the same subject. The perpendicular lines are drawn to show the method of comparing the several registrations. In this record the subject is stuttering, while in record C he is speaking normally. Note the tendency to attempt to speak when the lungs are empty, and even during the exhalation period.

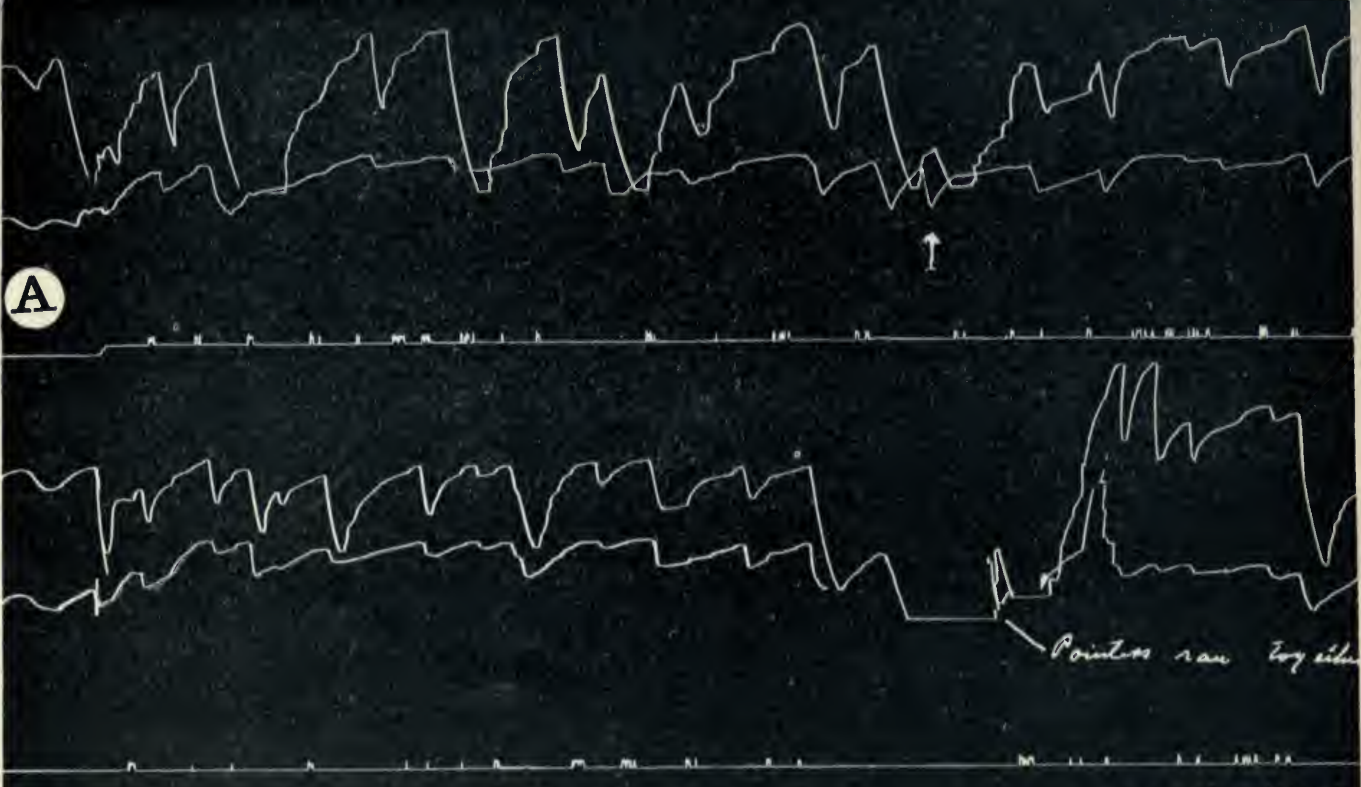
PLATE C is to be compared with record B. The regular succession of inhalation by exhalation, the short inhalation and the long exhalation period, also the complete utilization of the exhalation period by vocalization, as shown by the microphone record, are the chief points of difference between the two.

PLATE D gives a plethysmogram showing simultaneously the volumetric changes and the heart-rate, also the thoracic and the abdominal breathing curves. Sudden breathing and vaso-motor changes are seen to mark the period of the beginning of speech.

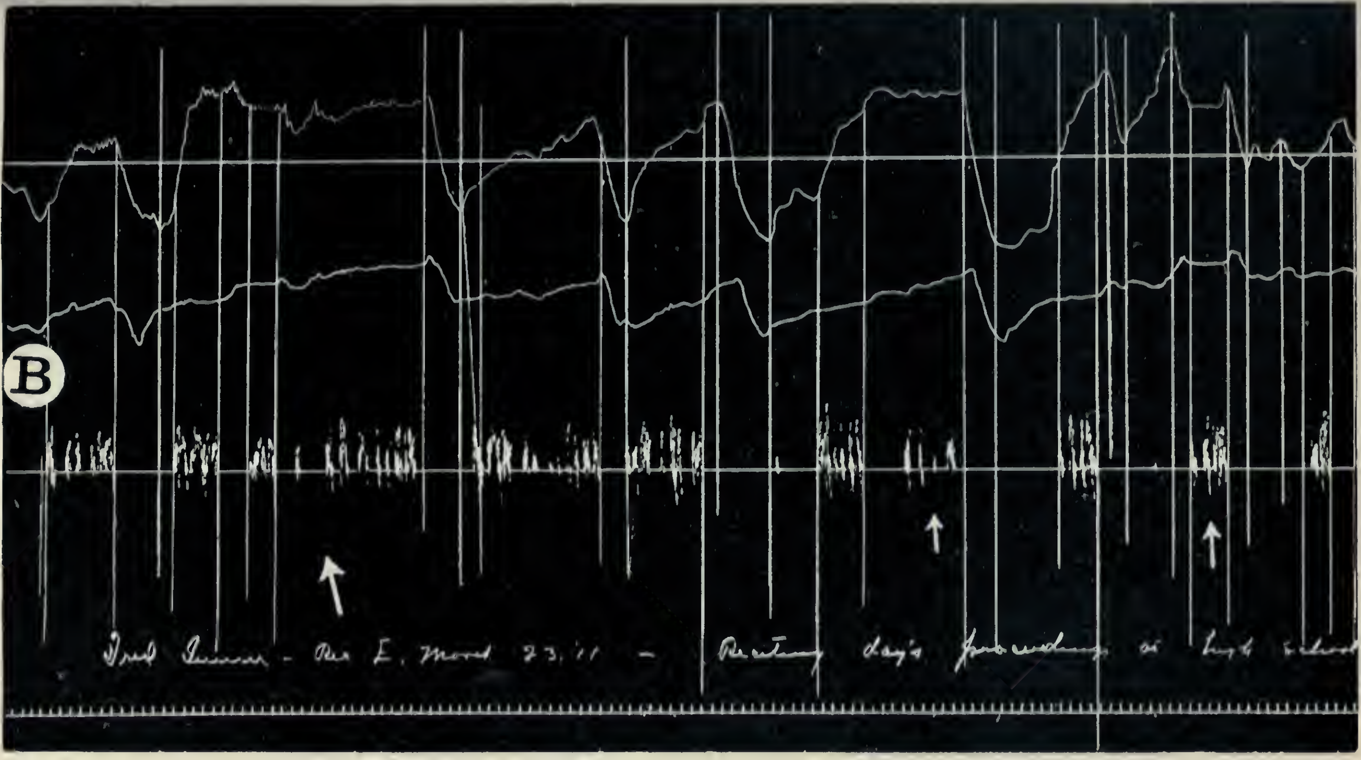
PLATE E gives the same as record D with the addition of a voice record. In addition to the marked irregularities of the breathing curve the steady rise of the plethysmographic curve is to be noted. The full extent of this rise is greater than could be shown in the length of the reproduction here given.

PLATE F shows the tracings that followed the deflections of the galvanometer during the stuttering of two subjects *H. A.* (top), and *A. N.* (bottom). *A. N.* is a much worse stutterer than *H. A.*, and there is also found in his case a much wider deflection. It is seen that *A. N.*'s curve reaches a maximum and then moves gradually in the direction of relaxation, or upward.

PLATE G shows the galvanometer deflections obtained from the same subject when reading under different conditions. In the first registration, *A. B.*, the subject, is reading alone and stuttering. In the second registration he is reading with the experimenter, α to β, without stuttering, and also relaxing, β to γ. The introspective report of this record showed the presence of "strain," and "lack of confidence" in the first registration, and "less emotional excitement" in the second.



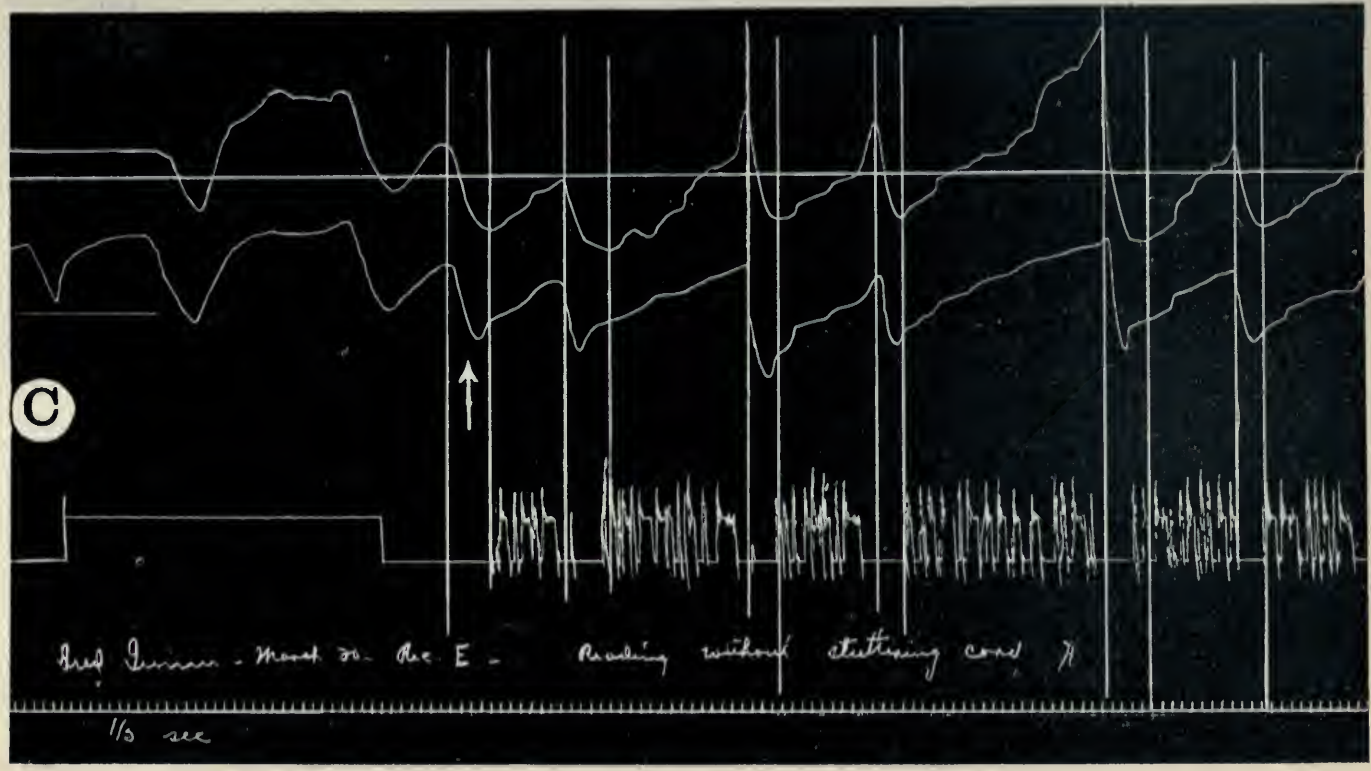
Albert Neale Nov. 1. 1911 Reading card 2 – Recorder registering voice faithfully.

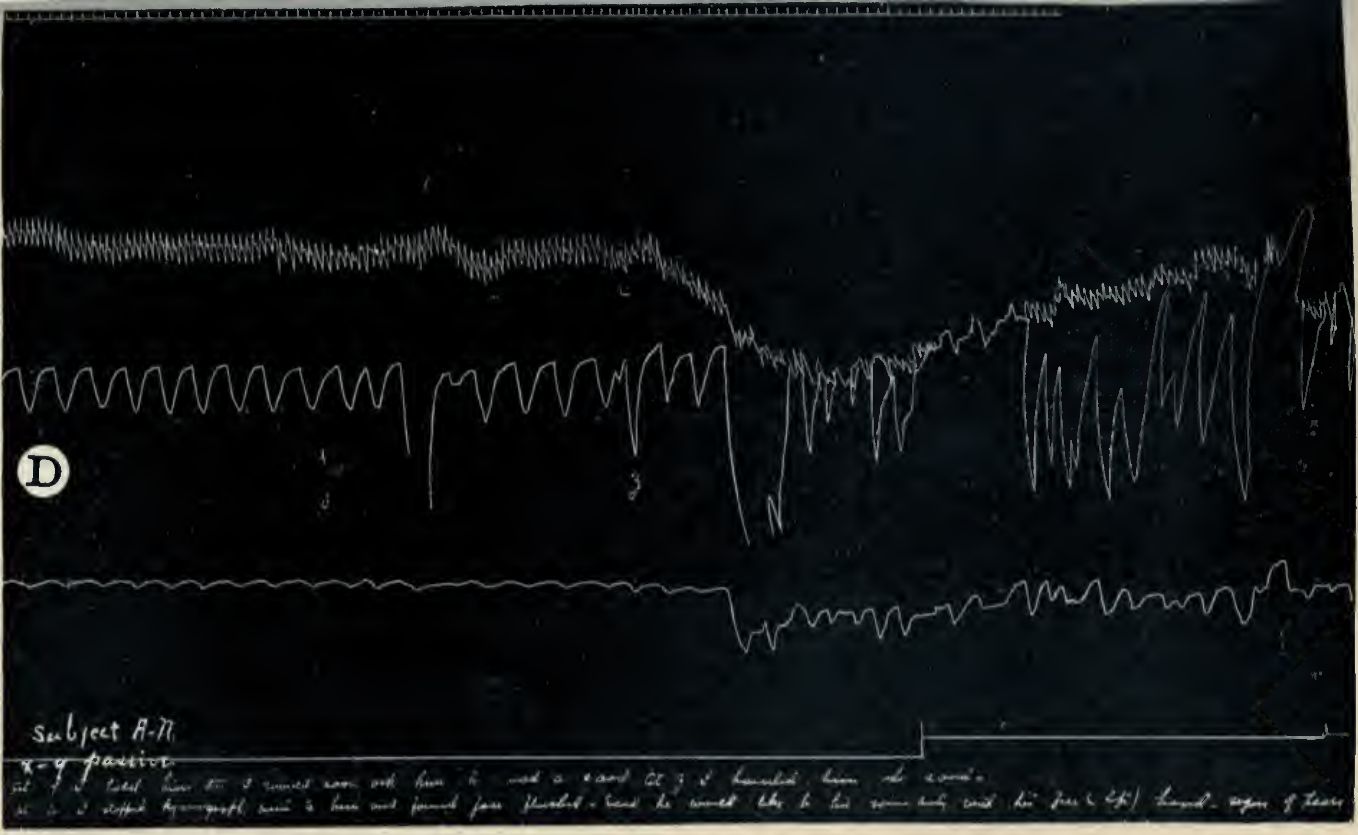


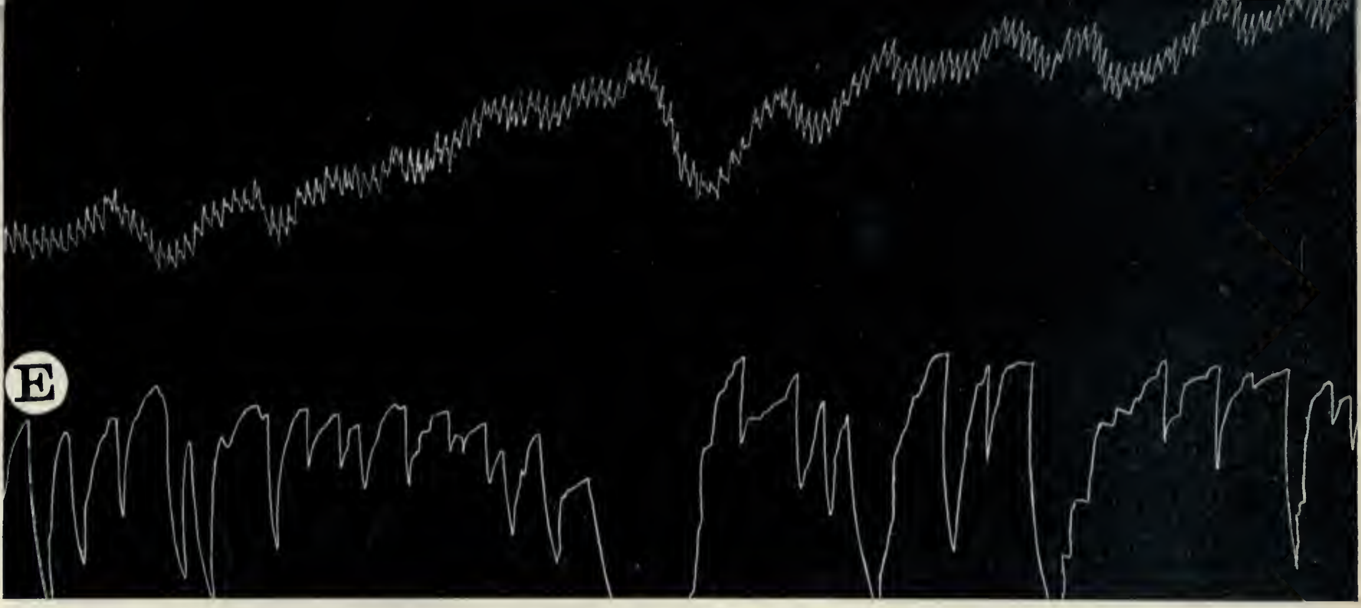
Fred Sumner - Rec E. Mard 23.11 - Reciting day's proceedings at high school

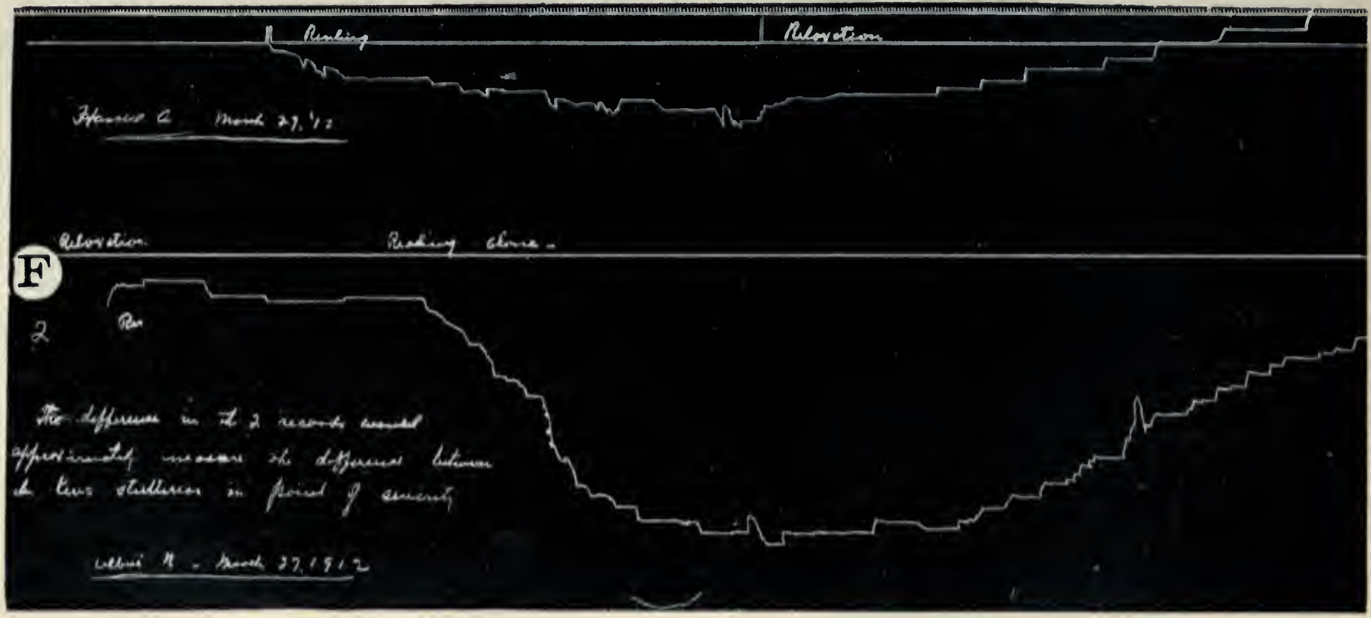
25-2

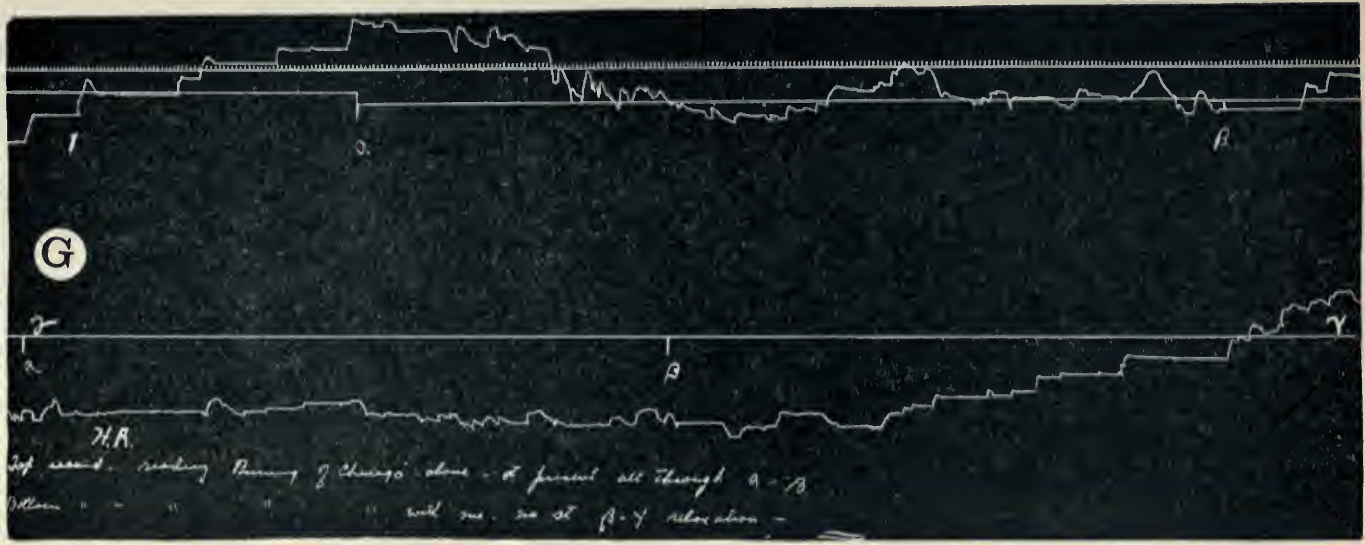
25·2⁴


C
Subj Inman - Masset. m. Rec. E - Reading without stuttering cond. R
1/5 sec


D
Subject A-M
x-y positive
at 1 I told him that I would now ask him to read a word at 3 I handed him the words.
at 4 he stopped [illegible] hand and [illegible] face flushed - said he would like to hit somebody [illegible] his face [illegible] [illegible] of tears


E

Reading
Relaxation
Francis C. March 27, '12
Relaxation.
Reading above.
F
2
Rx
The difference in the 2 records would approximately measure the difference between the two children in point of anxiety
Albert H. - March 27, 1912



Top record. reading Burney of Chicago alone – at present all through a – β

Bottom " – " " " with me. no at β-γ relaxation –

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THE INTERRELATIONS OF EMOTIONS AS SUGGESTED BY RECENT PHYSIOLOGICAL RESEARCHES*

By W. B. Cannon

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I. Introduction

The parts of the organism which are peculiarly activated in affective states are the glands and smooth muscle of the viscera. Although hurried breathing, and trembling, and characteristic bodily postures and facial expressions implicate the skeletal muscles, these muscles, under the will, may be sent into similar patterns without emotional experience, or in the throes of an emotional disturbance they may be set in deceptive patterns of quite opposite meaning—for example, we can smile through our tears. The pouring out of tears, however, and the "cold sweat," the dilation of the pupils, the erection of hairs, the pallor from contracted blood-vessels, the rapid heart—all these are changes from the routine of life, in organs not subject to voluntary control, in organs which are only roused thus from their normal even tenor by the rush of a wave of feeling. So characteristic are these disturbances of the viscera in affective states that they have been regarded, in the well known James-Lange theory, as giving rise to the peculiar qualities which differentiate emotional experiences from one another.

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* From the Laboratory of Physiology in the Harvard Medical School.

II. The General Organization of the Autonomic Nervous System

The glands and the smooth muscle of the viscera are, so far as is now known, never innervated directly from the central nervous system. Always there is interposed between the viscera and cerebrospinal neurones an extra neurone lying wholly outside the central nervous system. These outlying neurones,[1] with cell bodies grouped in ganglia (G's, Fig. 1) or distributed in the viscera themselves (see *e. g.,* heart and stomach, Fig. 1), form the so-called "autonomic system."[2] The neurones connecting the central nervous system with the peripheral ganglia do not pass out in a continuous series all along the cerebrospinal axis, but are separated by the roots of the nerve plexuses of the fore and hind limbs into three divisions. In front of the fore-limb plexus is the cranial division of the autonomic; between the fore-limb and the hind-limb plexuses is the thoracico-lumbar autonomic (or "sympathetic system," in the older terminology); and after the hind-limb plexus is the sacral autonomic. These three parts of the autonomic system have a number of interesting peculiarities.

1. The thoracico-lumbar autonomic is distributed throughout the body—to the eyes, the heart, the lungs, the digestive and genito-urinary tracts, and extensively to the walls of blood vessels and the smooth muscles of hairs.

2. The efferent fibres from the central nervous system may extend through several of the sympathetic ganglia and give off connections to cell bodies of the outlying neurones in each of them (note connections, Fig. 1). Although similar relations exist between afferent sensory fibres and cell bodies in a

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[1] Possibly these outlying neurones act as "transformers," modifying the impulses received from the central source, and adapting these impulses to the peculiar tissues—glands and smooth muscle—to which they are distributed.

[2] For most of our knowledge of the organization of this system we are indebted to Langley and his collaborators. See J. N. Langley: *Ergebnisse der Physiologie,* Wiesbaden 1903, II², p. 818, for summary.

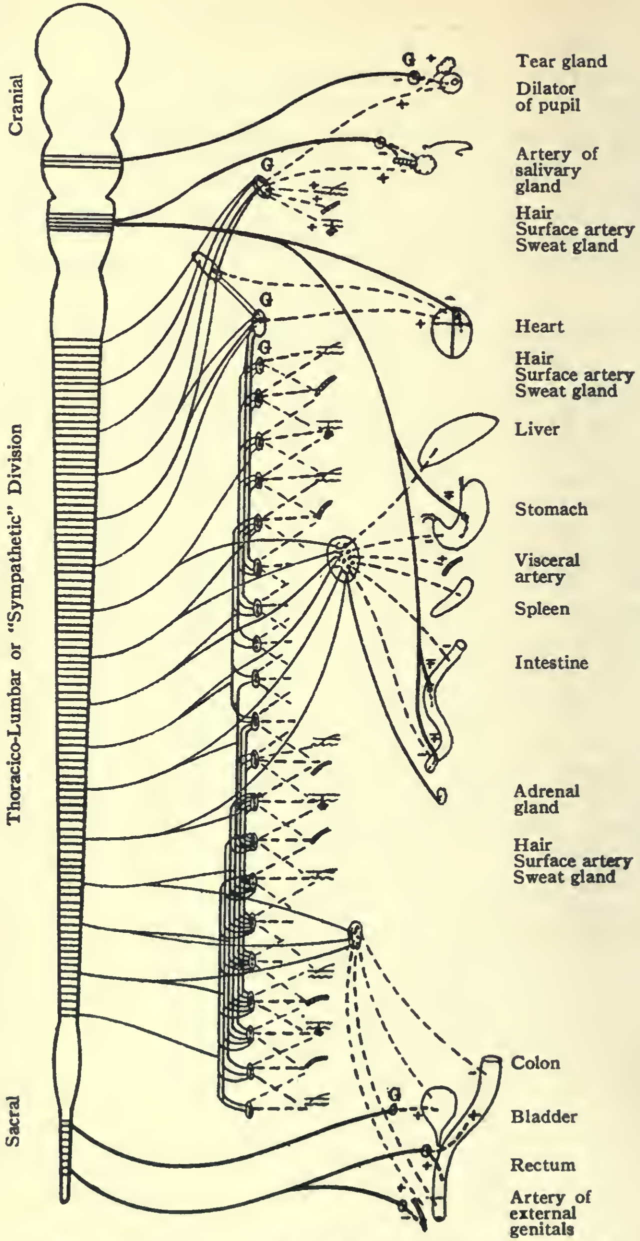

FIGURE 1.—Diagram of the more important distributions of the autonomic nervous system. The preganglionic fibres are in solid lines; the postganglionic in dash-lines. The nerves of the cranial and sacral divisions are distinguished from those of the thoracico-lumbar or "sympathetic" division by broader lines. A + mark indicates an augmenting effect on the activity of the organ; a — mark, a depressive or inhibitory effect.

series of segments in the spinal cord, the operation in the two cases is quite different. In the spinal cord the afferent impulse produces directed and closely limited effects, as, for example, when the left hind limb in a spinal animal flexes in response to a noxious stimulus applied to the left foot, without general involvement of the rest of the body in the response. In the action of the sympathetic, on the other hand, the numerous connections of single fibres with more peripheral neurones, seems to be not at all arranged for specific effects in this or that region. There are, to be sure, variations in the degree of functional involvement of different parts; for example, dilation of the pupil in the cat occurs more readily than erection of the hairs. It may be that specially direct pathways to the eye are present for common use in non-emotional states, and that only slight general disturbance in the nervous system would be necessary to send impulses by these well-worn courses. Thus tears might flow although other parts innervated by the sympathetic might be little disturbed. But we have no means of voluntarily wearing these pathways, and, in general, the neurone relations in the sympathetic seem devised for widespread diffusion of impulses.

3. The cranial and the sacral autonomic divisions differ from the thoracico-lumbar in having only restricted distribution; the cranial to the eyes, the heart, single arteries, the lungs, and the stomach and small intestine; the sacral to single arteries, the distal colon, the bladder and external genitals (see Fig. 1, cranial and sacral distribution).

4. The efferent fibres from the central nervous system to the sacral and cranial divisions of the autonomic have few of the distributed connections typical of the thoracico-lumbar division, but, like nerves to skeletal muscles, innervate separate organs (compare, *e.g.*, cranial and sympathetic supply to heart and stomach, Fig. 1). This singleness and separateness of connection is assured in many instances by the imbedding of the outlying neurones—as in the heart and stomach—in the body of the viscus itself.

5. As indicated in the foregoing lists, many of the viscera are innervated both by the sympathetic and by the cranial or sacral parts of the autonomic. *When the mid-part meets either end-part in any viscus they are antagonists.*[3] Thus the cranial supply to the eye contracts the pupil, the sympathetic dilates it; the cranial slows the heart, the sympathetic accelerates it; the sacral contracts the distal colon, the sympathetic

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[3] T. R. Elliott: The Action of Adrenalin. *The Journal of Physiology,* XXXII, 1905, 401-467. See pp. 424, 425.

relaxes it; the sympathetic contracts the urethra, the sacral relaxes it. These effects are indicated in Fig. 1 by + for contraction, acceleration, or increased tone, and by — for inhibition, relaxation, or decreased tone. The vagus nerve has a primary brief inhibitory effect on the stomach and small intestine ∓ ; its chief effect, however, is to produce contraction and increased tone in these organs. Probably these peripheral oppositions have counterparts in the organization of central neurones, just as antagonistic muscles are reciprocally innervated because of central arrangements, for Sherrington has noticed, and I can confirm the observation, that even though the sympathetic is prevented from acting the pupil dilates in a paroxysm of anger—due, no doubt, because the response is rapid, to central inhibition of the cranial supply to the constrictor muscles.

6. The adrenal glands produce a substance (adrenin, epinephrin, or adrenalin) which, in extraordinarily minute amounts, affects the structures innervated by the sympathetic precisely as if they were receiving nervous impulses. Thus adrenalin injected into the blood will cause dilated pupils, erect hairs, rapid heart, constricted bloodvessels, inhibited activities of the alimentary canal, and liberation of sugar from the liver.

7. The adrenal glands are innervated by preganglionic fibres of the autonomic group, and stimulation of these nerve fibres will cause a pouring out of adrenal secretion into the blood stream.[3a]

The foregoing brief sketch of the organization of the autonomic system brings out a number of points that should be of importance as bearing on the nature of the emotions which manifest themselves in the operations of this system. Thus it is highly probable that the sympathetic division, because arranged for diffuse discharge, is likely to be brought into activity as a whole rather than in parts. Also, because antagonisms exist between the middle and either end division of the autonomic, affective states may be classified according to their expression in the middle or an end division and these states would be, like the nerves, antagonistic in character. And finally, since the adrenal glands are innervated by autonomic fibres of the mid-division, and since adrenal secretion stimulates the same activities that are stimulated nervously by this division, it is possible that disturbances in the realm of the sympathetic, although initiated by nervous discharge, are automatically augmented and prolonged through chemical action. Some recent researches in which these considerations were kept in mind are now to be reported.

[3a] T. R. Elliott: The Innervation of the Adrenal Glands. *The Journal of Physiology,* XLVI, 1913, 289 ff.

III. The Operation of the Thoracico-Lumbar Autonomic Division in Pain, Fear and Rage

When a dog barks at a cat in confinement, the cat may strive to escape or crouch in a farther part of the cage, or it may rush forward in attack. The changes of smooth-muscle functions in the animal, either in crouching fear or in attacking rage, are much the same. The pupils are large, the motions of the stomach and intestines are inhibited, and the hair is erect on the back and tail—from one end to the other, the animal shows the effects of sympathetic innervation.[4] Do the adrenal glands share in this general excitation of the mid-division of the autonomic at times of great emotional stress?

This question was taken up by de la Paz and myself about three years ago.[5] We found that when a cat is frightened or enraged by a barking dog, blood near the opening of the adrenal veins gave definite evidence of the presence of adrenalin (tested by relaxation of the rhythmically-contracting intestinal strip), whereas blood from the same region previous to the excitement was ineffective. Later Hoskins and I found that strong stimulation of the sciatic nerve in an anaesthetized animal—such stimulation as would cause severe pain if the animal were not anaesthetized—resulted in greater activity of the adrenal medulla, as indicated by the increased amount of adrenalin in the blood.[6]

These observations on the effects of fear, rage and pain have been supported by Elliott's study of the adrenalin content of the adrenal glands as affected by experimental procedures.[7] He found that "fright," induced in cats by morphia or by β-tetrahydronaphthylamine, exhausts the glands, and that excitation of afferent nerves, such as the great sciatic, also causes adrenalin to disappear. These results are what could be reasonably expected, for major emotions, as fear and rage, and such sensory stimulation as in a conscious animal would be painful, are known to be accompanied by nerve impulses passing out via sympathetic fibres—impulses causing dilation of the pupils, inhibition of gastric peristalsis and secretion, and contraction of arterioles. And, as previously stated, the adrenal glands have been proved to manifest increased secretory activity when affected by nerve impulses coming via these same pathways.

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[4] W. B. Cannon: *The Mechanical Factors of Digestion,* London and New York, 1911, pp. 227. See p. 217.

[5] W. B. Cannon and D. de la Paz: Emotional Stimulation of Adrenal Secretion, *American Journal of Physiology,* XXVIII, 1911, 64-70. see p. 64.

[6] W. B. Cannon and R. G. Hoskins: The Effects of Asphyxia, Hyperpnoea, and Sensory Stimulation on Adrenal Secretion. *American Journal of Physiology,* XXIX, 1911, 274-279, see p. 278.

[7] T. R. Elliott: The Control of the Suprarenal Glands by the Splanchnic Nerves. *Journal of Physiology,* XLIV, 1912, p. 409.

Artificial stimulation of splanchnic nerves not only liberates adrenalin but also releases sugar from the liver.[8] Sugar is thus so greatly increased in the blood that it appears in the urine (glycosuria). If, however, the adrenals are removed from the body, splanchnic stimulation will not evoke glycosuria. The participation of the adrenal medulla, therefore, seems to be essential for the mobilization of sugar in the blood, when that is accomplished by nerve impulses.

Since adrenal secretion is increased in major emotional states and on stimulation of nerves for pain,—and since hyperglycaemia is the normal accompaniment of such experimental nervous stimulations as evoke an increased adrenal secretion, it might reasonably be expected that fear, rage and pain would give rise to hyperglycaemia.

That experimental procedures attended by pain result in the appearance of sugar in the urine was demonstrated many years ago by Böhm and Hoffman.[9] Their observations on cats have been corroborated on rabbits;[10] and recently it has been shown that an operation involving some pain increases blood sugar in dogs.[11]

That pure emotional excitement—fear or rage—will have the same effect, was proved when Shohl, Wright and I obtained glycosuria in cats by fastening them to a comfortable holder or by placing them in small cages and permitting a dog to bark at them. Whether glycosuria appeared promptly or not depended on the animal's emotional reaction to its experience. Neither pain nor being bound, therefore, was a factor in the result—the essential element was the fright or rage of the animal.[12] Our conclusion has been confirmed by one of my former students, Dr. W. G. Smillie, who found that four of nine medical students (all normally aglycosuric) had glycosuria after a very exacting examination, whereas only one of the nine had glycosuria after an easier examination.[13] Also Rolly and Oppermann,[14] Jacobsen,[15] and Hirsch and Reinbach[16] have recently reported that the mere handling of a rabbit preparatory to operating on it will increase the blood sugar (in some cases from 0.10 to 0.23 and 0.27 per cent) and may result in glycosuria. Indeed, the readiness with which this response occurs has been pointed out as a source of error in estimates of the "normal" sugar content of the blood.

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[8] J. J. R. Macleod: *Diabetes: its Pathological Physiology,* London, 1913, see pp. 61-62.

[9] R. Böhm and F. A. Hoffman: Beiträge zur Kenntniss des Kohlenhydratstoffwechsels. *Archiv für experimentelle Pathologie und Pharmakologie,* VIII, 1878, 271-308, see p. 295.

[10] C. Eckhard: Zur Deutung der Entstehung der vom vierten Ventrikel aus erzeugbaren Hydrusien. *Zeitschrift für Biologie,* XLIV, 1903, 407-440, see p. 408.

[11] A. Loewy and S. Rosenberg: Ueber die normale Höhe des Blutzuckergehalts bei Kaninchen und Hunden. *Biochemische Zeitschrift,* LVI, 1913, 114-116, see p. 114.

Fear, rage and pain, therefore, are accompanied by an increased discharge of adrenalin into the blood, and by a freeing of stored glycogen from the liver for circulation through the body as dextrose. What explanation can be offered for this remarkable outpouring from the adrenal medulla and the concomitant glycogenolysis that floods the body with sugar?

1. The Reflex Nature of Bodily Responses to Pain and the Major Emotions

The most significant feature of these bodily reactions to pain and to emotion-provoking objects is that they are of the nature of reflexes,—they are not willed movements, indeed they are often distressingly beyond the control of the will. The pattern of the reaction, in these as in other reflexes, is deeply inwrought in the workings of the nervous system, and when the appropriate occasion arises, typical organic responses are evoked through inherent automatisms.

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[12] W. B. Cannon, A. T. Shohl and W. S. Wright: Emotional Glycosuria. *American Journal of Physiology,* XXIX, 1911, 280-287, see p. 283.

[13] The tests were made on the first urine passed after the examination. Mr. C. H. Fiske and I have found sugar in the urine in 12 of 25 members of the Harvard University football squad, immediately after the final and most exciting contest of the season. Five of the positive cases were substitutes who were not called upon to enter the game.

[14] F. Rolly and F. Oppermann: Das Verhalten des Blutzuckers bei Gesunden und Kranken. *Biochemische Zeitschrift,* XLIX, 1913, 278-292, see p. 201.

[15] A. T. B. Jacobsen: Untersuchungen über den Einfluss des Chloralhydrats auf experimentelle Hyperglykaemieformen. *Ibid,* LI, 1913, 442-462, see p. 449.

[16] E. Hirsch and H. Reinbach: Die Fesselungshyperglykämie und Fesselungsglycosurie des Kaninchens. *Zeitschrift für physiologische Chemie,* LXXXVII, 1913, 122-141, see p. 137.

It has long been recognized that the most characteristic feature of reflexes is their "purposive" nature, or their utility either in preserving the welfare of the organism or in safeguarding it against injury. The reflexes of sucking, swallowing, vomiting and coughing, for instance, need only to be mentioned to indicate the variety of ways in which reflexes favor the continuance of existence. When, therefore, these automatic responses accompanying pain and fear and rage—the increased discharge of adrenalin and sugar—are under consideration, it is reasonable to inquire first as to their utility.

Numerous ingenious suggestions have been offered to account for the more obvious changes accompanying emotional states—as, for example, the bristling of the hair and the uncovering of the teeth in an access of rage. The most widely applicable explanation[17] proposed for these spontaneous reactions is that during the long course of racial experience they have been developed for quick service in the struggle for existence. McDougall[18] has suggested that an association has become established between peculiar emotions and these ingrained native reactions; thus the emotion of fear is associated with the instinct for flight, and the emotion of anger or rage with the instinct for fighting or attack. Earlier James had pointed out that "fear has bodily expressions of an extremely energetic kind, and stands, beside lust and anger, as one of the three most exciting emotions of which our nature is susceptible."[19]

Because the adrenalinaemia and the hyperglycaemia following painful or strong emotional experiences are reflex in character, and because reflexes as a rule are useful responses, we may reasonably enquire whether under these circumstances the increase of adrenalin and sugar in the blood is useful. What then is the possible value of these reactions?

In order that these reactions may be useful they must be *prompt*. Such is the case. Some recent observations have shown that the latent period of adrenal secretion, when the splanchnic nerve is stimulated below the diaphragm, is not longer than 16 seconds; and Macleod states that within a few minutes after splanchnic stimulation the sugar in the blood rises between 10 and 30 per cent.[20] The two secretions are, therefore, almost instantly ready for service.

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[17] Chas. Darwin: *Expression of Emotions in Man and Animals*, New York, 1905, pp. 374, see pp. 101 and 117.

[18] William McDougall: *Introduction to Social Psychology*, London, 1908, pp. 355, see pp. 49 and 59.

[19] William James: *Principles of Psychology*, New York, 1905, 2 vols., see II, p. 415.

Conceivably the two secretions might act in conjunction or each might function alone. Thus adrenalin might serve in cooperation with nervous excitement to produce increase of blood sugar, or it might have that function and other functions quite apart from that. Before these possibilities are considered, however, the value of the increased blood sugar itself will be discussed.

2. *The Utility of Increased Blood-Sugar*

In the paper on emotional glycosuria previously mentioned,[21] a clue was taken from McDougall's suggestion of a relation between "flight instinct" and "fear emotion," and "pugnacity instinct" and "anger emotion." And the point was made that, since the fear emotion, and the anger emotion are, in wild life, likely to be followed by activities (running or fighting) which require contraction of great muscular masses in supreme and prolonged struggle, a mobilization of sugar in the blood might be of signal service to the laboring muscles. Pain—and fighting is almost certain to involve pain—would, if possible, call forth even greater muscular effort. "In the agony of pain almost every muscle of the body is brought into strong action," Darwin wrote, for "great pain urges all animals, and has urged them during endless generations, to make the most violent and diversified efforts to escape from the cause of suffering."[22]

That muscular work is performed by energy supplied in carbonaceous material is shown by the great increase of carbon-dioxide output in severe muscular work, which may exceed twenty times the output during rest. Furthermore, the storage of glycogen in muscle, and the disappearance of this glycogen deposit from excised muscle stimulated to activity, or its reduction after excessive contractions produced by strychnine, and the lessened ability of muscles to work if their glycogen store has been reduced, and the simple chemical relation between sugar and the lactic acid which appears when muscles are repeatedly made to contract, are all indications that carbohydrate (sugar and glycogen) is the elective source of energy for contraction. This conclusion is supported in recent careful studies by Benedict and Cathcart,[23] who have shown that a small but distinct increase in the respiratory quotient occurs during muscular work, and that a decrease in the quotient follows, thus pointing to a larger proportion of carbohydrate burned during muscular work than before or after—*i. e.,* a call on the carbohydrate deposits of the body.

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[20] J. J. R. Macleod: *Loc. cit.*, see p. 80.

[21] W. B. Cannon, A. T. Shohl and W. S. Wright: *Loc. cit.*, see p. 286.

[22] Chas. Darwin: *Loc. cit.*, p. 72. It is recognized that both pain and the major emotions may have at times depressive rather than stimulating effects. Though severe pain may soon induce extreme prostration, the whip and spur illustrate its primary exciting action. And though fear may become the most depressing of all emotions, it acts at first as a powerful stimulus. "A man or animal driven through terror to desperation is endowed with wonderful strength, and is notoriously dangerous in the highest degree." (Darwin: *Loc. cit.*, p. 81).

Furthermore, there is considerable evidence that the increasing of blood sugar directly increases muscular efficiency. Thus Locke proved that if oxygenated salt solution is perfused through the rabbit heart, the beats begin to weaken after one or two hours; but if now 0.1 per cent dextrose is added to the perfusing fluid the beats at once become markedly stronger and may continue with very slow lessening of strength as long as seven hours.[24] And Schumberg noted that when he performed a large amount of general bodily work (thus using up blood sugar) and then tested flexion of the middle finger in an ergograph, the ability of the muscle was greater if he drank a sugar solution than if he drank an equally sweet solution of "dulcin."[25] He did not know during the experiment which solution he was drinking. These observations have been confirmed by Prantner and Stowasser, and by Frentzel.[26] In experiments on cats Lee and Harrold found that when sugar is removed from the animal by means of phlorhizin the *tibialis anticus* muscle is quickly fatigued; but if, after the phlorhizin treatment, the animal is given an abundance of sugar and then submitted to the test, the muscle shows a much larger capacity for work.[27] All this evidence is, of course, favorable to the view that circulating sugar may be quickly utilized by contracting muscles.

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[23] F. G. Benedict and E. R. Cathcart: *Muscular Work, a metabolic study,* Washington, 1913, pp. vi + 176, see pp. 85-87.

[24] F. S. Locke: Die Wirkung der Metalle des Blutplasma auf das isolierte Säugethierherz. *Centralblatt für Physiologie,* XIV, 1900, 670-672, see p. 671.

[25] Dr. Schumberg: Einfluss des Zuckergenuses auf die Leistungsfahigkeit der Musculatur. *Archiv für Physiologie, Jahrgang,* 1896, 537-538, see p. 537.

[26] Johannes Frentzel: Ergographische Versuche über die Nährstoffe als Kraftspender für ermüdete Muskeln. *Archiv für Physiologie, Jahrgang,* 1899, Supplement Band, 141-159, see p. 145.

[27] Frederic Lee and C. C. Harrold: The Action of Phlorhizin on Muscle, Proceedings of the Amer. Physiological Society, 5th Special Meeting. *American Journal of Physiology,* IV, 1900, ix-x.

Experimental studies have shown, then, that muscles work preferably by utilizing the energy stored in sugar, that great muscular labor is capable of considerably reducing the quantity of stored glycogen and of circulating sugar, and that under circumstances of a lessened sugar content the increase of blood sugar considerably augments the ability of muscles to continue contracting. The conclusion seems justified, therefore, that the increase of blood sugar attendant on the major emotions and pain, is of direct benefit to the organism in the strenuous muscular efforts involved in flight or conflict or struggle to be free.

3. The Utility of Increased Adrenalin in the Blood

In early work on the effects of removal of the adrenal bodies muscular weakness was not infrequently noted. In 1892, Albanese showed that muscles stimulated after adrenalectomy were much more exhausted than when stimulated the same length of time in the same animal before the removal.[28] Similarly Boinet reported that rats recently deprived of their adrenal glands were much more quickly exhausted in a revolving cage than were normal animals.[29] A beneficial effect of adrenal extract on fatigued muscle, even when applied to the solution in which the isolated muscle is contracting, was claimed by Dessy and Grandis,[30] who studied the phenomenon in the salamander.

It seemed possible, because of the early evidence that adrenalectomy has a debilitating effect on muscular power, and that injection of adrenal extract has an invigorating effect, that increased adrenal secretion, as a reflex result of pain or the major emotions, might not only be useful in helping to mobilize sugar, but also might act in itself as a dynamogenic factor in the performance of muscular work. On the basis of this possibility Nice and I[31] tested the effect of stimulating the left splanchnic nerve (thus causing adrenal secretion), or injecting adrenalin, on the contraction of the fatigued *tibialis anticus*. We found that when arterial pressure was of normal height, and was prevented from rising in the legs while the splanchnic nerve was being stimulated, there was a distinct rise in the height of contraction of the fatigued muscle. We drew the inference that adrenalin set free in the blood may operate favorably to the organism by preparing fatigued muscles for better response to the nervous discharges sent forth in great excitement.

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[28] M. Albanese: La fatigue chez les animaux privés des capsules surrénales. *Archives italiennes de biologie,* XVII, 1892, 239-247, see p. 243.

[29] E. Boinet: Résistance à la fatigue de 11 rats décapsulés, depuis cinq et six mois. *Comptes rendus, la Societe de Biologie,* Tome II, 10. Serie, XLVII de la collection, 1895, 162-167. Nouvelles recherches sur la résistance à la fatigue de rats décapsulés depuis longtemps. *Ibid.,* 325-326.

[30] S. Dessy and V. Grandis: Contribution à l'étude de la fatigue. Action de l'adrenaline sur la fonction du muscle. *Archives italiennes de biologie* XLI, 1904, 225-233, see p. 231.

This inference has been further tested by one of my students, Mr. C. M. Gruber,[32] who has examined the effects of minute amounts of adrenalin (0.1 or 0.5 c. c. of 1:100,000), and also of splanchnic stimulation, on the threshold stimulus of fatigued neuromuscular and muscular apparatus. Fatigue raises the threshold not uncommonly 100 or 200 per cent and in some instances as much as 600 per cent. Rest will restore the normal threshold in periods varying from 15 to 120 minutes, according to the length of previous stimulation. If a small dose of adrenalin is given, however, the normal threshold may be restored in three to five minutes.

From the foregoing evidence the conclusion is warranted that adrenalin, when freely liberated in the blood, not only aids in bringing out sugar from the liver's store of glycogen, but also has a remarkable influence in quickly restoring to fatigued muscles, which have lost their original irritability, the same readiness for response which they had when fresh. Thus the adrenalin set free in pain and in fear and rage would put the muscles of the body unqualifiedly at the disposal of the nervous system; the difficulty which nerve impulses might have in calling the muscles into full activity would be practically abolished; and this provision, along with the abundance of energy-supplying sugar newly flushed into the blood, would give to the animal in which these mechanisms are most efficient the best possible conditions for putting forth supreme muscular efforts.

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[31] W. B. Cannon and L. B. Nice: The Effect of Adrenal Secretion on Muscular Fatigue. *American Journal of Physiology*, XXXII, 1913, 44-60, see p. 54.

[32] C. M. Gruber: Studies in Fatigue III. The Fatigue Threshold as affected by Adrenalin and by Increased Arterial Pressure. *American Journal of Physiology*, XXXIII, 1914, 333-355, see p. 354.

4. *The Utility of the Vascular Changes Produced by Adrenalin*

Quite in harmony with the foregoing argument that sugar and adrenalin, which are poured into the blood during emotional excitement, render the organism more efficient in the physical struggle for existence, are the vascular changes wrought by increased adrenalin, probably in coöperation with sympathetic innervations. Through oncometric studies, Oliver and Schäfer[33] proved that the viscera of the splanchnic area —as the spleen, the kidneys, and the intestines—suffer a considerable diminution of volume when adrenalin is administered, whereas the limbs into which the blood is forced from the splanchnic region, actually increase in size. In other words, at times of stress, blood may be driven out of vegetative organs of the interior, which serve the routine needs of the body, into the skeletal muscles, which have to meet by extra action the urgent demands of conflict.

But there are exceptions to the statement that by adrenalin the viscera are emptied of their blood. It is well known that adrenalin has a vasodilator, not a vasoconstrictor, effect on the arteries of the heart; it is well known also that adrenalin affects the intracranial and the pulmonary vessels only slightly, if at all.[34]

Thus the absolutely essential organs—the "tripod of life" (heart, lungs, and brain) as well as the skeletal muscles —are, in times of excitement, when the adrenal glands discharge, abundantly supplied with blood taken from organs of less importance in critical moments.

The cessation of activities of the alimentary canal (thus freeing the energy supply for other parts); the shifting of the blood from the less insistent abdominal viscera to the organs immediately vital, such as the lungs, the heart, the central nervous system, and, at critical moments, the skeletal muscles as well; the increased cardiac vigor; the quick abolition of the effects of muscular fatigue; the mobilizing of energy-giving sugar in the circulation—these are the changes which occur when in fear or rage or pain the adrenal glands are made to pour forth an excessive secretion. These changes doubtless represent to some degree the reservoirs of power which, as James suggested,[34a] exist in our bodies and are ready to be tapped in times of need. The changes are, each one of them, *directly serviceable in making the organism more efficient in the struggle which fear or rage or pain may involve;* for fear and rage are aspects of organic preparations for action, and pain attends conditions which naturally evoke supreme exertion. And the organism which with the aid of increased adrenal secretion can best muster its energies, best call forth sugar to supply the laboring muscles, best lessen fatigue, and best send blood to the parts essential in the run or the fight for life, is most likely to survive.

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[33] George Oliver and E. A. Schäfer: The Physiological Effects of Extracts of the Suprarenal Capsules. *Journal of Physiology,* XVIII, 1895, 230-276, see p. 240.

[34] A. Biedl: *Innere Sekretion,* Second Edition, Leipzig, 1913, 2 vols., see I, pp. 434, 435.

IV. The Operation of the Cranial Autonomic Division in the Pleasures of Appetite

The well-known researches of Pawlow[35] have made clear the importance of appetite and the relish of food in causing secretion of the digestive glands. The mere showing of desirable food to a dog causes the saliva and the gastric juice to be poured forth in a true psychic secretion. And if the dog has an opening in the oesophagus so that food does not go to the stomach, the repeated chewing and swallowing of appetizing morsels (" sham feeding ") is accompanied by constant and prolonged production of gastric juice. All these glands—both the salivary group and those of the gastric wall —are innervated by cranial autonomic fibres (See Fig. 1). Section of these fibres abolishes the psychic secretion.

These observations on dogs have been almost completely confirmed by studies of human beings having oesophageal obstruction and an opening through the body wall into the stomach. Hornborg,[36] Bogen[37] and others have reported such cases in detail. Hornborg found that when the boy whom he examined chewed agreeable food a more or less active secretion of gastric juice was started, whereas the chewing of indifferent material was without influence.

In studying the mechanical aspect of digestion also, I was led to infer that there is probably a "psychic tone" or "psychic contraction" of the muscles as a result of taking food. For if the vagus autonomic fibres are severed immediately before food is taken into the stomach, the usual contractions of the gastric wall do not occur; but if these fibres are severed after food has been eaten with relish the contractions which have started continue without cessation. Thus, just as the secretions are started by pleasurable taste and smell, the conditions for peristalsis may also have a basis in mild affective states.[38]

[34a] William James: *Energies of Man,* New York, 1910.

[35] J. P. Pawlow: *The Work of the Digestive Glands.* London, 1902.

[36] A. F. Hornborg: Beiträge zur Kenntniss der Absonderungsbedingungen des Magensaftes beim Menschen. *Skandanavisches Archiv für Physiologie,* XV, 1904, 209-256, see p. 248.

[37] H. Bogen: Experimentelle Untersuchungen über psychische und associative Magensaftsecretion beim Menschen. *Archiv für die gesammte Physiologie,* CXVII, 1907, 150-160, see p. 156.

Aside from positive effects on the muscles of the alimentary tract and its accessory glands, cranial autonomic fibres cause contraction of the pupil of the eye, and slowing of the heart rate.

A glance at these various functions of the cranial division shows at once that they serve for bodily conservation—for the shielding of the retina from excessive light, for the resting and upbuilding of the heart, and for the taking in and storing of energy-bearing material through proper digestion and absorption.

V. The Operation of the Sacral Autonomic in Reflexes for Emptying

Sacral autonomic fibres cause contraction of the rectum and distal colon and also contraction of the bladder. In both instances the effects result from stretching of the tonically contracted viscera by their accumulating contents. No affective states precede the normal actions of the sacral division and even those which accompany or follow them are only mildly positive; a feeling of relief rather than of elation usually attends the completion of the act—there is, however, testimony to the contrary.

The sacral autonomic fibres also include the nervi erigentes which bring about engorgement of erectile tissue in the external genitals. According to Langley and Anderson[39] the sacral nerves have no effect on the *internal* generative organs. The vasa deferentia and the seminal vesicles whose rhythmic contractions mark the acme of sexual excitement in the male, and the uterus whose contractions in the female are probably analogous, are supplied only by lumbar branches—part of the sympathetic division. These branches also act in opposition to the nervi erigentes and cause constriction of the blood vessels of the external genitals. The sexual organism involves a high degree of emotional excitement; but it can be rightly considered as essentially a reflex mechanism; and, again in this instance, distention of tubules, vesicles and blood vessels can be found at the beginning of the incident, and relief from this distension at the end.

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[38] W. B. Cannon: *The Mechanical Factors of Digestion.* See p. 200.

[39] J. N. Langley and H. K. Anderson: The Innervation of the Pelvic and Adjoining Viscera, Parts II-V. *Journal of Physiology,* XIX, 1895, 71-139, see pp. 85, 122.

Although distention is the commonest occasion for bringing the sacral division into activity it is not the only occasion. Great emotion, such as is accompanied by nervous discharges via the thoracico-lumbar division may also be accompanied by discharges via the sacral fibres. The involuntary voiding of the bladder and lower gut at times of violent mental stress is well known. Veterans of our Civil War testify that just before the beginning of a battle many of the men had to retire temporarily from the firing line. And the power of sights and smells and libidinous thoughts to disturb the regions controlled by the nervi erigentes proves that this part of the autonomic system also has its peculiar affective states. The fact that one part of the sacral division, *e. g.*, the distribution to the bladder, may be in abeyance while another part, *e. g.*, the distribution to the rectum, is active, illustrates in this division the directive rather than the diffuse discharge of impulses which has been previously described.

Like the cranial division, the sacral is engaged in internal service to the body, in the performance of acts leading immediately to greater comfort.

VI. The Antagonism of Affective States Expressed in Opposed Divisions of the Autonomic System

Affective states gain expression through nervous discharges along autonomic pathways. When the nerve fibres of the mid-division of the autonomic system, however, meet in any organ the nerve fibres of either of the end divisions, the two sets are antagonists. As stated above, there is evidence that central arrangements exist for reciprocal innervation of these antagonistic divisions, just as there is reciprocal innervation of antagonistic skeletal muscles. The characteristic affective states manifested in these different divisions have been described. Undoubtedly these states have correspondents in the central neurones. The question now arises, are the states which appear in opposed divisions also in opposition?

1. Antagonism between the Thoracico-lumbar and Cranial Divisions

The cranial autonomic, as already shown, is concerned with the quiet service of building up reserves, and fortifying the body against times of stress. Accompanying these functions are the relatively mild pleasures of the sight and taste and smell of food. The possibility of existence of these gentle delights of eating and drinking and also of their physiological consequences is instantly swept away by affective states which activate the sympathetic division.

The stoppage of salivary flow and the consequent drying of the mouth, in fear and great anxiety, have been made familiar in the ordeal of rice. Failure of gastric secretion also has been demonstrated. Thus Hornborg was unable to confirm in his patient Pawlow's observation that mere showing of food to a hungry subject causes a flow of gastric juice. The difference in results was due to difference in the reaction of the subjects to the situation. When food was shown, but withheld, Pawlow's hungry dogs were all eagerness to secure it, and the juice at once began to flow. Hornborg's little boy, on the contrary, became vexed when he could not get the food, and began to cry; then no secretion appeared. Bogen also reported that his patient, aged about four years, sometimes fell into such a passion after vain hoping for food that the giving of food after calming the child, was not followed by any secretion of gastric juice.

The observations of Bickel and Sasaki[40] confirm and define more precisely the inhibitory effects of violent emotion on gastric secretion. In their control observations sham feeding was attended by a copious flow of gastric juice, a "psychic secretion," resulting from the pleasurable taste of the food. In a typical instance the sham feeding lasted five minutes, and the secretion continued for twenty minutes, during which time 66.7 c. c. of pure gastric juice was produced.

On another day a cat was brought into the presence of the dog, whereupon the dog flew into a great fury. The cat was soon removed, and the dog pacified. Now the dog was again given the sham feeling for five minutes. In spite of the fact that the animal was hungry and ate eagerly, there was no secretion worthy of mention. During a period of twenty minutes, corresponding to the previous observation, only 9 c. c.

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[40] A. Bickel and F. Sasaki: Experimentelle Untersuchungen über den Einfluss von Affekten auf die Magensaftsekretion. *Deutsche medizinische Wochenschrift,* XXXI, 1905, 1829-1831, see p. 1829.

of acid fluid was produced, and this was rich in mucus. It is evident that in the dog, as in the boy observed by Bogen, strong emotions can so profoundly disarrange the mechanisms of secretion that the natural nervous excitation accompanying the taking of food cannot cause the normal flow.

On another occasion Bickel and Sasaki started gastric secretion in the dog by sham feeding, and when the flow of gastric juice had reached a certain height, the dog was infuriated for five minutes by the presence of the cat. During the next fifteen minutes there appeared only a few drops of a very mucous secretion. Evidently in this instance a physiological process, started as an accompaniment of a psychic state quietly pleasurable in character, was almost entirely stopped by another physiological process accompanied by a psychic state violent in character.

Not only are the secretory activities of the stomach unfavorably affected by strong emotions; but, as already indicated, the movements of the stomach and, indeed, the movements of almost the entire alimentary canal are wholly stopped during excitement. In my earliest observations on the movements of the stomach I[41] had difficulty, because in some animals the peristalsis was perfectly evident and in others there was no sign of activity. Several weeks passed before I discovered that this difference in response to the presence of food in the stomach was associated with a difference of sex: the male cats were restive and excited on being fastened to the holder, and under these circumstances gastric peristalsis was absent; the female cats, especially if elderly, submitted with calmness to the restraint, and in them peristaltic waves took their normal course. Once a female with kittens turned from her state of quiet contentment to one of apparent restless anxiety. The movements of the stomach immediately stopped, and only started again after the animal had been petted and had begun to purr. I later found that by covering the cat's mouth and nose with the fingers until a slight distress of breathing occurred, the stomach movements could be stopped at will. Thus, in the cat, any sign of rage, such as Bickel and Sasaki's dog manifested, or distress, or mere anxiety, was accompanied by a total cessation of the movements of the stomach.

Just as the secretory activity of the stomach is affected in similar fashion in man and in lower animals, so likewise gastric and intestinal peristalsis are stopped in man as they are stopped in the lower animals, during worry and anxiety and the major affective states. Indeed the feeling of heaviness in the epigastrium commonly complained of by nervous persons may be due to inactivity of the canal and to stagnation of food. That such stagnation occurs in human beings has been proved by examination of the gastric contents after excitement, when the meal eaten twelve hours or more previously has been found still present.

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[41] W. B. Cannon: The Movements of the Stomach Studied by Means of the Röntgen Rays. *American Journal of Physiology*, I, 1898, 359-382, see p. 380.

All these disturbances of digestion seem mere interruptions of the "normal" course of events unless the part they may play in adaptive reactions is considered. In discussing the operations of the sympathetic division I pointed out that all the bodily changes which occur in the intense emotional states —such as fear and fury—occur as results of sympathetic discharges, and are in the highest degree serviceable to the organism in the struggle for existence likely to be precipitated when these emotions are aroused. From this point of view these perturbations which so readily seize and dominate the organs commonly controlled by the cranial autonomic are bodily reactions which may be of the utmost importance to life at times of critical emergency. Thus are the body's reserves—the stored adrenalin, and the accumulated sugar—called forth for instant service; thus is the blood shifted to nerves and muscles that may have to bear the brunt of struggle; thus is the heart set rapidly beating to speed the circulation; and thus, also, are the activities of the digestive organs for the time abolished. Just as in war between states the arts and industries which have brought wealth and contentment must suffer serious neglect or be wholly set aside both by the attacker and the attacked, and all the supplies and energies developed in the period of peace must be devoted to the coming conflict; so, likewise, the functions which in quiet times establish and support the bodily reserves are, in times of stress, instantly checked or abolished, and these reserves lavishly drawn upon to increase power in the attack and in the defence or flight.

It is, therefore, the natural antagonism between these two processes in the body—between conservation and liberation, between anabolism and catabolism—and the correlated antagonism of central innervations (see p. 259), that underlie the antipathy between the affective states which normally accompany the processes. The desire for food, the relish of eating it, all the pleasures of the table, are naught in the presence of anger or great anxiety. And of the two sorts of affective states those which manifest themselves in the dominant division of the autonomic, hold the field also in consciousness.

2. *Antagonism between the Thoracico-lumbar and Sacral Divisions*

The nervi erigentes are the part of the sacral autonomic in which the peculiar excitements of sex are expressed. As previously stated, these nerves are opposed by branches from the thoracico-lumbar division—the division which is operated characteristically in the major emotions.

The opposition in normal individuals between the affective states which appear in these two antagonistic divisions is most striking. Even in animals as low in the scale as birds copulation is not performed " until every condition of circumstance and sentiment is fulfilled, until time, place and partner all are fit."[42] And among men the effect of fear or momentary anxiety or any intense emotional interest in causing inhibition of the act can be supported by cases in the experience of any physician with extensive practice.[42a]

As the acme of excitement is approaching it is probable that the thoracico-lumbar division is also called into activity; indeed, the completion of the process—the contractions of the seminal vesicles and the prostate, and the subsidence of engorged tissues, all innervated by sympathetic filaments (see pp. 271-2)—may be due to the overwhelming of sacral by thoracico-lumbar nervous discharges. As soon as this stage is reached the original feeling likewise has been dissipated.

The other parts of the *sacral* division which supply the bladder and rectum are so nearly free from any affective tone in their normal reflex functioning that it is unnecessary to consider them further with reference to emotional antagonisms. Mild affective states, such as worry and anxiety, can, to be sure, check the activity of the colon and thus cause constipation.[42b] But the augmented activity of these parts (contraction of the bladder and rectum) in very intense periods of emotional stress, when the *thoracico-lumbar* division is strongly innervated, presents a problem the solution of which I do not yet perceive. Possibly in such conditions the orderliness of the central arrangements is upset, just as it is after tetanus toxin or strychnine poisoning, and opposed innervations no longer discharge reciprocally, but simultaneously, and then the stronger member of the pair prevails. Only on such a basis, at present, can I offer any explanation for the activity and the supremacy of the sacral innervation of the bladder and distal colon when the thoracico-lumbar innervation is aroused as, for example, in great fright.

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[42] William James: *Loc. cit.*, I, p. 22.

[42a] The pathological cases in which rage or other violent excitement is reported as preceding or following the act are difficult to reconcile with the view here propounded. On careful examination, however, these cases may be found not to contravene the conditions of physiological antagonism in the opposed parts of the autonomic that are excited.

[42b] A. F. Hertz: *Constipation and Allied Intestinal Disorders*, London, 1909, see p. 81.

VII. The Similarity of Visceral Effects in Different Strong Emotions and Suggestions as to its Psychological Significance

A summary in few words of the chief functions typically performed or supported by each division of the autonomic would designate the cranial division as the upbuilder and restorer of the organic reserves, the sacral as the servant of racial continuity, and the thoracico-lumbar as the preserver of the individual. Self-preservation is primary and essential; on that depends racial continuity and for that all the resources of the organism are called forth. Analogously the sympathetic innervations, when they meet in organs innervated also by the cranial and sacral divisions, almost with exception predominate over their opponents. And analogously also, the emotional states which are manifested in the thoracico-lumbar division and are characteristically much more intense than those manifested in the other divisions, readily assume ascendency also in consciousness.

These dominant emotions are fear and rage, and they are not unlike. As James has indicated, " Fear is a reaction aroused by the same objects that arouse ferocity. . . . We both fear, and wish to kill anything that may kill us; and the question which of the two impulses we shall follow is usually decided by some one of those *collateral circumstances* of the particular case, to be moved by which is the mark of superior mental natures."[43] The cornering of an animal when in the headlong flight of fear may suddenly turn the fear to fury and the flight to a fighting in which all the strength of desperation is displayed.

Furthermore these dominant emotions are states into which many other commonly milder affective states may be suddenly transformed. As McDougall has pointed out, all instinctive impulses when met with opposition or obstruction give place to, or are complicated by, the pugnacious or combative impulse directed against the source of the obstruction.[44] A dog will bristle at any attempt to take away his food, males will fight furiously when provoked by interference with the satisfaction of the sexual impulse, a man will forget the conventions and turn hot for combat when there is imputation against his honor, and a mother all gentle with maternal devotion is stung to quick resentment and will make a fierce display of her combative resources, if anyone intentionally injures her child. In these instances of thwarted or disturbed instinctive acts the emotional accompaniments—such as the satisfactions of food and of sexual affection, the feeling of self-pride, and the tender love of a parent—are whirled suddenly into anger. And anger in one is likely to provoke anger or fear in the other who for the moment is the object of the strong feeling of antagonism. Anger is the emotion pre-eminently serviceable for the display of power, and fear is often its counterpart. Darwin testifies to having heard "as a proof of the exciting nature of anger, that a man when excessively jaded will sometimes invent imaginary offences and put himself into a passion, unconsciously for the sake of reinvigorating himself"; and Darwin continues, "since hearing this remark, I have occasionally recognized its full truth."

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[43] William James: *Loc. cit.*, II, see p. 415.

The visceral changes which accompany fear and rage have been previously described as the result of discharges by way of sympathetic fibres. So far as these two quite different emotions are concerned, present physiological evidence indicates that differences in the bodily accompaniments are not noteworthy.[45] And there is, indeed, obvious reason why the visceral changes in fear and rage should not be *different,* but rather, why they should be *alike.* As already pointed out, these emotions accompany organic preparations for action, and just because the conditions which evoke them are likely to result in flight or conflict (either one requiring, perhaps, the utmost struggle) the bodily needs in either response are precisely the same.

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[44] Wm. McDougall: *Loc. cit.,* see p. 72.

[45] Obvious vascular differences, as pallor or flushing of the face, are of little significance. With increase of blood pressure from vasoconstriction, pallor might result from action of the constrictors in the face, or flushing might result because constrictors elsewhere, as, for example, in the abdomen, raised the pressure so high that facial constrictors are overcome. Such, apparently, is the effect of adrenalin, already described (see p. 269). Or the flushing might occur from local vasodilation. That very different emotional states may have the same vascular accompaniments was noted by Darwin (*Loc. cit.*) who mentioned the pallor of rage (p. 74) and also of terror (p. 77).

In discussing the functioning of the thoracico-lumbar division I pointed out that it was roused to activity not only in fear and rage, but also in pain. The machinery of this division likewise is operated wholly or partially in emotions which are usually mild—such as joy and sorrow and disgust—*when they become sufficiently intense.* Thus, for instance, the normal course of digestion may be stopped or quite reversed in a variety of these emotional states.

Darwin reports the case of a young man who on hearing that a fortune had just been left him, became pale, then exhilarated, and after various expressions of joyous feeling vomited the half-digested contents of his stomach.[46] Müller has described the case of a young woman whose lover had broken the engagement of marriage. She wept in bitter sorrow for several days, and during that time vomited whatever food she took.[47] And Burton, in his *Anatomy of Melancholy,* gives the following instance of the effect of disgust:[48] "A gentlewoman of the same city saw a fat hog cut up, when the entrails were opened, and a noisome savour offended her nose, she much disliked, and would not longer abide; a physician in presence told her, as that hog, so was she, full of filthy excrements, and aggravated the matter by some other loathsome instances, insomuch this nice gentlewoman apprehended it so deeply that she fell forthwith a vomiting, was so mightily distempered in mind and body, that with all his art and persuasion, for some months after, he could not restore her to herself again, she could not forget or remove the object out of her sight."

In these three cases, of intense joy, intense sorrow and intense disgust, the influence of the cranial division of the autonomic has been overcome, digestion has ceased, and the stagnant gastric contents by reflexes in striated muscles have been violently discharged. The extent to which under such circumstances other effects of thoracico-lumbar impulses may be manifested, has not, so far as I know, been ascertained.

From the evidence just given it appears that any high degree of excitement in the central nervous system, whether felt as anger, terror, pain, anxiety, joy, grief or deep disgust, is likely to break over the threshold of the sympathetic division and disturb the quiet of all the organs which that division innervates. It may be that there is advantage in the readiness with which these widely different emotional conditions can express themselves in this one division, for, as has been shown (see p. 264), occasions may arise when these milder emotions are suddenly transmuted into the naturally intense types (as fright and fury) which normally activate this division; and if the less intense can also influence it, the physiological aspect of the transmutation is already partially accomplished.

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[46] Chas. Darwin: *Loc. cit.,* see p. 76.

[47] Müller: Klinische Beiträge zur Physiologie des sympathetischen Nervensystems. *Deutsches Archiv für klinische Medicin,* LXXXIX, 1907, 432-456, see p. 434.

[48] Robert Burton: *The Anatomy of Melancholy* (first published in 1621), London, 1886, P + 1, p. 443.

If various strong emotions can thus be expressed in the diffused activities of a single division of the autonomic—the division which accelerates the heart, inhibits the movements of the stomach and intestines, contracts the blood vessels, erects the hairs, liberates sugar, and discharges adrenalin—it would appear that the bodily conditions which have been assumed, by some psychologists, to distinguish emotions from one another must be sought for elsewhere than in the viscera. We do not " feel sorry because we cry," as James contended, but we cry because, when we are sorry or overjoyed or violently angry or full of tender affection—when any one of these diverse emotional states is present—there are nervous discharges by sympathetic channels to various viscera, including the lachrymal glands. And in terror and rage and intense elation, for example, the responses in the viscera seem too uniform to offer a satisfactory means of distinguishing states which, in man at least, are subjectively very different. For this reason I am inclined to urge that the visceral changes merely contribute to an emotional complex more or less indefinite, but still pertinent, feelings of disturbance, in organs of which we are not usually conscious.

This view that emotions are not of visceral origin is in accord with the experimental results of Sherrington,[49] who has demonstrated that emotional responses occur in dogs in which practically all the main viscera and the great bulk of skeletal muscle have been removed from subjection to and from influence upon the brain, by severance of the vagus nerves and the spinal cord. In these animals no alteration whatever was noticed in the occurrence, under appropriate circumstances, of expressions of voice and features, indicating anger, delight or fear. The argument that these expressions may have been previously established by afferent impulses from excited viscera was met by noting that a puppy only nine weeks old also continued to exhibit the signs of affective states after the brain was disconnected from all the body except head and shoulders. Evidence from uniformity of visceral response and evidence from exclusion of the viscera are harmonious, therefore, in pointing towards central rather than peripheral changes as the source of differences in emotional states.

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[49] Charles S. Sherrington: Experiments on the Value of Vascular and Visceral Factors for the Genesis of Emotion. *Proceedings of the Royal Society,* LXVI, 1900, 390-403, see p. 397.

If these differences are due to central changes, why is it not always possible by voluntary innervations to produce emotions? We can laugh and cry and tremble. But forced laughter does not bring happiness nor forced sobbing sorrow, and the trembling from cold rouses neither anger nor fear. The muscle positions and tensions are there, but the experiencing of such bodily changes does not seem even approximately to rouse an emotion in us. The *voluntary* assumption of an attitude seems to leave out the "feeling." It is probable, however, that no attitude which we can assume has all the elements in it which appear in the complete response to a stirring situation. But is not this because the natural response is a pattern reaction, like inborn reflexes of low order, such as sneezing, in which impulses flash through peculiarly cooperating neurone groups of the central system, suddenly, unexpectedly, and in a manner not exactly reproducible by volition, and thus they throw the skeletal muscles into peculiar attitudes and, if sufficiently intense, rush out in diffuse discharges that cause tremors and vascular perturbations? The typical facial and bodily expressions, automatically assumed in different emotions and so well known as to constitute the common language between man and beast, indicate the discharge of peculiar groupings of neurones in the several affective states. That these responses occur instantly and spontaneously when the appropriate "situation," actual or vividly imagined, is present, shows that they are ingrained in the nervous organization. At least one such pattern, that of anger, persists after removal of the hemispheres,—the decorticated dog, by growling and biting when handled, has the appearance of being enraged;[50] the decerebrate cat when vigorously stimulated retracts its lips and tongue, stares with dilated pupils, snarls, and snaps its jaws.[51] On the other hand, stroking the hair, whistling, gently calling, or yelling to produce fright have not the slightest effect in evoking from the decorticated dog, signs of joy, affection or fear, nor does the animal manifest any sexual feeling. The absence of bodily indications of these emotions is quite as significant as the presence of the signs of anger. For, since expressions of anger can persist without the cortex, there is little reason why the complexes of other emotional expressions, if their " machinery " exists below the cortex, should not also be elicitable. That they are not elicitable suggests that they require a more elaborately organized grouping of neurones than does anger—possibly what the cortex, or the cortex in combination with basal ganglia, would provide.

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[50] Fr. Goltz: Der Hund ohne Grosshirn. *Archiv für die gesammte Physiologie,* LI, 1892, 570-614, see p. 577.

[51] R. S. Woodworth and C. S. Sherrington: A Pseudaffective Reflex and its Spinal Path. *Journal of Physiology,* XXXI, 1904, 234-243, see p. 234.

The contrast between the brevity of the " pseudoaffective reactions " in the decerebrate cat, though the viscera are still connected with the central nervous system, and the normal duration of emotional expression in the dog with body separated from the head region, has been used by Sherrington to weigh the importance of the visceral and cerebral factors. And for reasons given above, as well as for the reasons he has offered, I agree with Sherrington's conclusion " that the reverberation from the trunk, limbs and viscera counts for relatively little, even in the primitive emotions of the dog, as compared with the cerebral reverberation to which is adjunct the psychical component of emotional reaction."[52]

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[52] Charles S. Sherrington: *The Integrative Action of the Nervous System,* New York, 1906, pp. xvi + 411, see p. 268.

THE ARTICULATION OF THE CONCEPTS OF NORMAL AND ABNORMAL PSYCHOLOGY[1]

By Jared S. Moore, Ph.D., Western Reserve University

That abnormal psychology bears the same relation to normal that pathology bears to physiology, that the fundamental concepts of psychology and psychopathology are identical, would seem to be a truism; and yet the reader of ordinary psychological literature turning to some of the recent works on mental pathology—as from a file of The Psychological Review to some late issue of The Journal of Abnormal Psychology—cannot fail to be impressed by the differences in terminology and point of view between these two branches of our general science as they stand today. Above all, the doctrine of the complex, which so permeates contemporary discussions of mental disorders, finds no place at all in treatises on the normal mental processes, so that the student who takes his elementary course in psychology in the college, and later enters upon the study of the abnormal mind, finds it necessary to learn the foundations of the science all over again, instead of naturally pressing on, as he would do in any other science, from already established rudiments to a more advanced and specialized study based on those same rudiments. And yet, as a matter of fact, if the doctrine is true at all it is first a theory of the *mind* before it is a theory of the *diseased* mind; and, further, its very value for abnormal psychology consists in its capacity to explain mental disorders as due to the perverted functioning of the same complexes which in their normal functioning constitute the healthy mind. A complete understanding of mental disorders, therefore, involves an understanding of the complex as a normal factor in mental life—that is to say, involves the study of the normal mind as an integration of complexes.

Mind as the psychologist views it is defined structurally as a sum-total of contents, dynamically as a sum-total of processes. The individual contents or processes we find almost universally classified under three heads—cognitive, affective, and conative, to use the terms now most in favor; the distinctive feature of cognition consisting in its reference to an object, of in affection its ascription to that object of a value for the self, and of conation in its tendency to motor expression. All psychologists agree that there is no rigid division between these three groups, but that all three aspects are discoverable in every moment of consciousness and in every content of such a moment, though one of the three will in every case predominate. Analysis reveals also that all mental contents are complexes of elements, and that no mental element ever occurs alone. In other words, the unit of mental life is not the mental element, nor a complex of mental elements of one kind, but a complex possessing at one time a cognitive basis, an affective value, and a conative trend—in a word, the unit of mental life is just The Complex, in the Freudian sense of that term.

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[1] Read in part before the American Psychological Association at the New Haven meeting, Dec. 31, 1913.

The psychological problem generated by this doctrine is threefold—structural, genetic, and dynamic. The structural analysis of the complex reveals but two constituent factors—cognitive and affective: conation is not a structural factor, but the dynamic aspect of the complex, that phase of it which expresses the fact that the complex is no dead thing, but an active force seeking outward expression in the physical world through the medium of motor activity. The cognitive factor is itself complex, consisting of a group of associated ideas, each idea in turn being composed of a number of elements: as contrasted with this there can be but one affective element, or, at least, only one of each dimension of affective elements—pleasant or unpleasant, exciting or quieting, etc. Thus the complete structural analysis of the complex reduces it finally to a group of cognitive elements combined into ideas, *plus* the single "affect," as may be symbolized in the following diagram:—

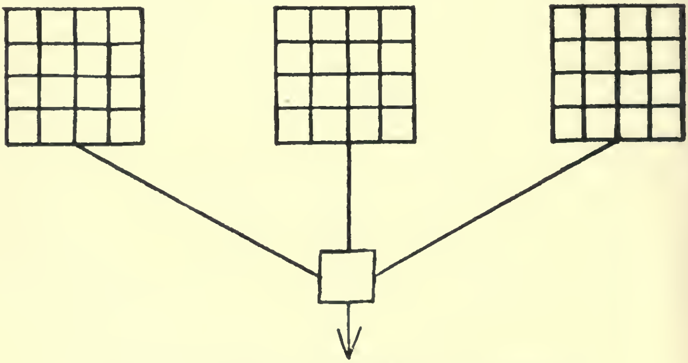

FIG. 1—Structural Analysis of the Complex

Here the three squares with their subdivisions represent the associated ideas with their constituent elements, the lower small undivided square the single affect, and the arrow the conative trend of the complex.

In this treatment of the complex we have a common meeting-ground for structuralist and functionalist. The structuralist is primarily interested in the analysis of mental contents, and this analysis is always into two groups of elements—cognitive and affective. The problem of whether there are one or more sub-groups of cognitive elements—whether one or more pairs of affective elements—is a secondary problem whose solution one way or another does not affect our general program. In any case the structuralist recognizes that volitional contents do not introduce any new element, but that will, like feeling and the different forms of knowledge, is constituted solely of cognitive and affective elements.

The functionalist, on the other hand, is primarily interested in the dynamic value of the mental processes, in conation rather than cognition and affection. To him knowledge and feeling are not so much contents of mind as processes expressing themselves in action, as media between stimulus and reaction having for their chief purpose the controlling of the adjustments between the organism and its environment. There is no conflict, then, between structuralist and functionalist, but merely a difference in the emphasis each lays on one or another phase of mental life, and the doctrine of the complex is first of all of value in its articulation of these two points of view.

Heretofore our treatment has been histological—from the complex to its elements: we pass now in the other direction from the individual complex to the complete personality. If a group of ideas with their affect are integrated in the complex, it is natural to think of these complexes as again integrated into systems, these into systems of a higher order, and so forth, the final integration of all the systems constituting what we know as the personality. Personality, then, is an integration of systems of complexes, and in the completely normal personality the various complexes and systems will be so interrelated that it is possible to pass smoothly and easily from one to another. This is represented in Fig. 2 (see p. 286).

On its genetic side it is the task of psychology to trace (1) the development of the complex out of the mass of cognitive and affective elements—the "complication" of old and new cognitive elements in the idea, of each idea with its group of associated ideas, and of all these with their appropriate affect; and (2) the growth of the personality by the accretion of new complexes. Finally, in its dynamic aspect, as we have seen, psychology studies the conative tendencies and efficient activities of the complex, with particular reference to the value of these conative tendencies for the personality as a whole—whether in furtherance of the personal integration or in conflict with it.

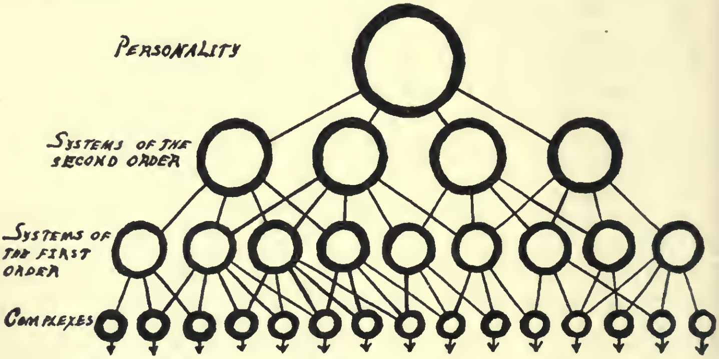

FIG. 2—Structural Analysis of Normal Personality

On the foundation of these two concepts, therefore, of the Complex, and Personality as an integration of complexes, a complete science of psychology may be built up, and the present unfortunate hiatus between normal and abnormal, and structural and functional, psychology happily bridged. Let the student be introduced early in his introductory course to the notion of the complex, and he will not be obliged to relearn the rudiments, or rather to learn an entirely new alphabet, of the science when he considers it later on in its pathological aspect. If in addition the psychopathologist can be prevailed upon to give back to the normal psychologist the stolen concept "Psychosis," allowing it to signify *any* temporary status of the personality, still more can be done in the direction of articulation.

In the execution of this eirenic program normal psychology will properly claim for its province the study of the general nature and internal constitution of complexes, psychoses, and personalities, of the processes of complication and conservation, and of all complex-activities which further the personal integration. I do not say that the average undergraduate student is fitted to enter at once upon the study of all these phenomena, but only that his introduction to the science should be such that he may afterwards enter easily upon the more advanced treatment without a feeling that he is studying an entirely new science, as is certainly the case under our present system. Finally, the field of normal psychology being thus delimited, it falls to the psychopathologist to discuss such disturbing processes as conflict, repression, and dissociation. The normal condition is symbolized in Fig. 2, in which every complex and system is connected continuously with every other, and the conative tendencies of all, as represented by the arrows, are harmonious, or in fulfillment of the personal integration. In contrast to this, a morbid condition would be represented on its structural side by a group of complexes not connected with the rest of the personality, and in its dynamic aspect by differently pointed arrows, thus:—

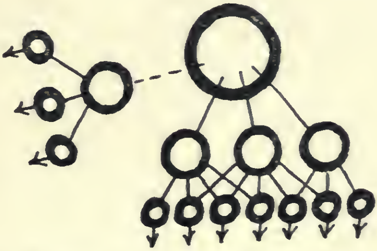

FIG. 3—Analysis of a Psychopathic Personality

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

XXIV. The Speed of Affective Judgments

By Helen M. Potter, Ruth Tuttle, and M. F. Washburn

The object of this study was to compare the average times required to make the judgments 'indifferent,' 'pleasant,' 'unpleasant,' 'very pleasant,' and 'very unpleasant,' using colored papers as the sources of affective reaction, and measuring time with a stop-watch. A piece 2.9 cm. square was cut from each of the Bradley colors, and these pieces were laid one at a time on a sheet of white paper before the observer. In order that no time should be lost in looking for the piece of paper, it was always placed on a certain mark previously located by the observer. Her eyes were closed except when the signal was given to open them. The experimenter gave a 'Ready' signal as she put the piece of paper in place; when she said 'Now,' she started a stop-watch, and the observer opened her eyes and looked at the color. As soon as the observer expressed a judgment on the affective character of the color impression, the stop-watch was stopped. The usual series of numbers from one to seven was not employed to express the affective judgment. It seemed best to use a less finely graded scale, for with the seven-place scale there may be hesitation in any part of the scale as to just which number to use. Instead, we instructed our observers to decide simply whether a color was pleasant, unpleasant, or indifferent to them, but they were told that whenever a color struck them as *very* pleasant or unpleasant, they might record the fact. The judgments of extreme pleasantness and unpleasantness were by these instructions made more spontaneous, since there was ordinarily no obligation to decide on the degree of the affective tone. Ninety reactions were thus taken with each observer, and there were fifty-five observers, all young women college students.

For each observer the average reaction time was calculated for judgments of indifference, of extreme pleasantness, and of extreme unpleasantness. Through a misunderstanding, one of the two experimenters calculated the average reaction time for all the pleasant judgments including judgments of extreme pleasantness, and for all the judgments of unpleasantness including extreme unpleasantness, while the other experimenter found the average reaction time for judgments of extreme pleasantness, that for judgments of moderate pleasantness, that for judgments of extreme unpleasantness, and that for judgments of moderate unpleasantness, separately. Each experimenter worked with a considerable number of observers, and since either way of treating the results leads to the same conclusion, it was not thought worth while to reduce one set of numbers to the terms of the other set.

The results, then, were as follows:

Experimenter P. Thirty observers. The average of the average reaction times of *judgments of extreme pleasantness* was 1.2 seconds; the longest individual average was 1.5 seconds; the shortest individual average was .9 seconds. The average of the average reaction times for *judgments of extreme unpleasantness* was 1.2 seconds; the longest individual average was 2.0 seconds, and the shortest .9 seconds. The average of the average reaction times for *all the judgments of pleasantness* taken together was 1.4 seconds; longest individual average 2.3, shortest 1. The average of the average reaction times for *all the judgments of unpleasantness* taken together was 1.4; longest individual average 2.5, shortest 1. The average of the average reaction times for *judgments of indifference* was 1.8 seconds; longest individual average 2.5, shortest 1.1.

Thus from the results of Experimenter P we find (1) that there is no difference whatever between the average time required for judgments of pleasantness and that required for judgments of unpleasantness; (2) that it takes on the average about .2 of a second less time to make a judgment of either extreme pleasantness or extreme unpleasantness than to make judgments of pleasantness or unpleasantness including both moderate and extreme grades; (3) that it takes about .4 of a second longer to make the judgment 'indifferent' than to make a judgment of pleasantness or unpleasantness.

Experimenter T. Twenty-five observers. The average of the average reaction times of *judgments of extreme pleasantness* was 1.3 seconds; the longest individual average was 1.9 seconds, the shortest .8 of a second. The average of the average reaction times for *judgments of extreme unpleasantness* was 1.3 seconds; the longest individual average was 2 seconds and the shortest .8 of a second. The average of the average reaction times for *judgments of pleasantness exclusive of extreme judgments* was 1.6 seconds; longest individual average 2.7 seconds, shortest .9 of a second. The average of the average reaction times for *judgments of unpleasantness exclusive of extreme judgments* was 1.6 seconds; longest individual average 2.4, shortest .9 of a second. The average of the average reaction times for *judgments of indifference* was 1.9 seconds; longest individual average 3.3, shortest 1 second.

From the results of Experimenter T it may be concluded (1) that there is no difference whatever between the average time required for judgments of pleasantness and that required for judgments of unpleasantness; (2) that it takes on the average about .3 of a second longer to make a judgment of moderate pleasantness or unpleasantness than to make an extreme judgment under either category; (3) that it takes about .3 of a second longer to make a judgment of indifference than to make one of moderate pleasantness or unpleasantness.

These results seem mutually confirmatory. That the averages are not misleading may be shown by a study of the individual results. For forty-six observers out of fifty-five the average reaction time for judgments of indifference was longer than any of the other averages, and for four of the remaining nine it was equal to the longest average. Twenty-four out of thirty observers gave shorter averages for judgments of extreme pleasantness than for judgments of both grades of pleasantness taken together, and seventeen out of twenty-five gave shorter averages for judgments of extreme pleasantness than for judgments of moderate pleasantness. Twenty-three out of twenty-nine observers gave shorter averages for extreme judgments of unpleasantness than for unpleasantness in general (one observer never made a judgment of extreme unpleasantness), and nineteen out of twenty-five gave shorter averages for extreme than for moderate judgments of unpleasantness. When we take into consideration the roughness of the time measurements and the many sources of possible variation in the results, their uniformity appears to be significant.

There are two different points of view, at least, from which these facts may be regarded. On the one hand, it may be urged that the stronger the affective tone of a stimulus, the more motor energy it represents; hence we should expect a strong and prompt motor response from very pleasant or very unpleasant stimuli, a weaker one from stimuli of moderate affective tone, and a still weaker one from indifferent stimuli. Nakashima,[1] however, found that reactions involving affective tone take longer than ordinary cognition or discrimination reactions, a result which he ascribes to the lack of clearness as an attribute of affection. If this explanation be accepted it may be argued that extreme degrees of pleasantness and unpleasantness would possess in their intensity a kind of substitute for clearness, and so would give shorter reaction times than moderate degrees, but it might seem that we should be unable to account for the fact that judgments of indifference took regularly longer than judgments of pleasantness and unpleasantness. Of course, though, a judgment of indifference is a very different thing from a cognitive judgment about a stimulus that is as a matter of fact indifferent. In the latter case attention is not directed to the existence or non-existence of an affective tone; in the former case the problem is: Has this stimulus produced an affective experience or has it not?

A largely contributing factor to our results is probably the same law that makes associative reactions to ambiguous words slower than those to *eindeutig* words. The indifference judgments take longest because there are three alternatives more or less present to consciousness, namely, pleasantness, unpleasantness, and indifference; the judgments of moderate pleasantness and unpleasantness take fairly long because the possibility of the opposite judgment or of an indifference judgment still lurks in the background; while the extreme judgments are the shortest because there is no alternative possibility present at all to consciousness. The indifference judgments, further, probably take longer than the judgments of moderate pleasantness and unpleasantness because the Aufgabe involves the discovery if possible of an affective tone, and many observers feel that an indifference judgment means a failure to perform the Aufgabe, despite the fact that the instructions allow it.

The effect of practice rather than fatigue was apparent in most cases when the time of the first thirty reactions was compared with that of the last thirty. In forty-three out of fifty-five cases the last thirty reactions were quicker than the first thirty. Moreover, practice tended to level somewhat the differences between the averages for the different categories of judgment.

XXV. A STUDY OF AFFECTIVE CONTRAST.

By MARGARET M. BACON, ESTHER A. ROOD, and M. F. WASHBURN

The pleasure of an agreeable experience is heightened if it is preceded by a disagreeable experience, and an impression in itself unpleasant may be felt as pleasant if a more unpleasant state has been its antecedent. In like manner unpleasantness may be heightened or even created through contrast with a preceding agreeable affective state. These are laws of the affective life which every-day experience has established. We undertook to investigate them under laboratory conditions.

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[1] *Psychological Review,* vol. 16, 1909, pp. 303-340; *American Journal of Psychology,* vol. 20, 1909, pp. 157-193.

As in the earlier experiments performed in this laboratory on affective processes, our materials were pieces 2.9 cm. square of the ninety Bradley colors. As a preliminary, the entire set of ninety pieces was spread on the table before the observer, who was asked to pick out six colors which seemed to her extremely disagreeable, and six which seemed to her extremely pleasant. These were set aside, the experimenter making a note of their numbers. Eighteen other colors were then chosen by the experimenter, saturated colors, shades, and tints being equally represented. These eighteen colors were shown to the observer, one at a time, on a white ground. Between the showing of each color and that of the next, one of the pleasant colors selected by the observer was shown. As each color was presented, the observer was asked to express her judgment of its pleasantness or unpleasantness by using one of the numbers from 1 to 7 in the usual way. When all of the eighteen colors had been shown, each preceded by one of the very pleasant colors, the experiment was continued without pause and the same eighteen colors were again presented, in a different order, each this time preceded by one of the very unpleasant colors chosen by the observer. Care was taken that the second presentation of a color should not follow its first presentation by so short an interval that the observer would of necessity recall her first judgment of its affective value. If affective contrast exerted a decided influence, the affective values of the colors seen immediately after an unpleasant color ought to be higher than the values of the same colors seen immediately after a pleasant color.

In order to equalize fatigue conditions, with other observers the colors were first presented preceded by the unpleasant colors and later preceded by the pleasant ones.

One of the experimenters (E. A. R.) told her observers that the object of the experiment was to study the influence which the preceding pleasant and unpleasant colors would have upon their judgments of the affective value of the colors, and the observers were acquainted of course with the general facts of affective contrast. The other experimenter (M. M. B.) gave her observers no information whatever about the experiment, and a number of them did not even realize that the same colors were being presented twice. Thus E. A. R. used a method 'with knowledge' and M. M. B. a method 'without knowledge.' This difference was most interestingly influential upon the results.

E. A. R., whose observers worked with knowledge, experimented upon forty-seven persons. In the case of thirty, the unpleasant 'inducing' colors were used first and the pleasant ones second. Affective contrast, if present as an influence, should have brought about a lowering of the affective values of the colors on their second presentation. In the case of five observers, more affective values were lowered on second presentation than were either stationary or raised. In the case of five other observers, more affective values were raised on the second presentation than were either stationary or lowered. The other twenty observers gave the same affective value for the majority of the colors in both presentations. Of these, however, nine had more lowered judgments than raised judgments, six had more raised than lowered, four had an equal number of judgments raised and lowered, and one had her judgments equally distributed among the 'raised,' 'lowered,' and 'stationary' classes.

Seventeen of E. A. R.'s observers had the pleasant inducing colors presented first and the unpleasant ones second. Affective contrast should here have brought about a raising of affective values on the second presentation of the colors. Seven observers had more judgments raised than either remained stationary or were lowered. Two had more lowered than either were raised or remained stationary The other eight gave the same affective judgment for both presentations in the case of the majority of the colors, but of these two observers had more values raised than lowered, five had more lowered than raised, and one had an equal number lowered and raised.

In thus appears that no effect of contrast is demonstrable in E. A. R.'s results. Only fourteen out of thirty observers gave more lowered than raised affective judgments on colors presented immediately after very pleasant ones, and only nine out of seventeen observers gave more raised than lowered judgments on colors that were presented immediately after very unpleasant ones.

M. M. B., whose observers worked without knowledge of the purpose of the experiments, obtained strikingly different results. She experimented upon thirty-seven persons. In the case of twelve of these, the unpleasant inducing stimuli were given first and the pleasant ones second. Here affective contrast should bring about a lowering of the affective values assigned the colors on their second presentation as compared with the first. Eleven of the twelve observers had more affective values lowered on the second presentation than were either raised or left unchanged. The figures are impressive. They are as follows: *Observer 1*, 12 colors lowered, 2 raised, 4 unchanged; *observer 2*, 12 colors lowered, none raised, 6 unchanged; *observer 3*, 11 lowered, 1 raised, 6 unchanged, *observer 4*, 16 lowered, none raised, 2 unchanged; *observer 5*, 15 lowered, none raised, 3 unchanged; *observer 6*, 8 lowered, 2 raised, 8 unchanged; *observer 7*, 11 lowered, none raised, 7 unchanged; *observer 8*, 11 lowered, none raised, 7 unchanged; *observer 9*, 18 lowered, none raised and none unchanged; *observer 10*, 13 lowered, none raised, 5 unchanged; *observer 11*, 13 lowered, none raised, 5 unchanged; *observer 12*, 16 lowered, none raised, 2 unchanged.

For the other twenty-five of M. M. B.'s observers, the pleasant inducing stimuli were given first; the effect of affective contrast here should have been to raise the values of the colors on their second presentation. Twelve out of the twenty-five had more affective values raised on the second presentation than were either lowered or left unchanged. Eleven had more unchanged judgments than either raised or lowered judgments, but all of these had more raised than lowered. The other two had more judgments lowered than were either raised or left unchanged.

These results certainly demonstrate the influence of affective contrast. Every observer who had the colors presented first after unpleasant and later after pleasant colors had more affective values lowered than raised by the process; twenty-three out of twenty-five observers to whom the colors were shown first after pleasant and later after unpleasant colors had more affective judgments raised than lowered on the second presentation.

That this contrast effect showed itself strikingly only in those observers who were working without knowledge that it might be looked for is a fact more interesting than the contrast influence itself. The knowledge that one might be expected to find a color unusually agreeable if one had just seen a very disagreeable color, or *vice versa,* instead of aiding the natural contrast influence by the force of positive suggestion, seems merely to have confused and obliterated it. Whether this was because the observers resisted the suggestion we cannot say, without introspective data which could not in the nature of the case have been furnished. Suggestion does have a positive influence on the pleasantness and unpleasantness of colors, even when it is given in the form of direct verbal suggestion, which is most apt to stir up opposition. It seems, however, that affective reactions are so delicately adjusted that we cannot merely add one positive influence to another and look for a summation of effects. The factor of affective contrast is most strongly operative when the affective judgments are made on the bare sense impressions, without any extraneous influence.

XXVI. THE CORRELATION BETWEEN ACCURACY OF THE VISUAL MEMORY AFTER-IMAGE AND CONTROL OF VISUAL IMAGERY

By HELEN ADLER, MYRA WILLIAMS, and M. F. WASHBURN

We have long outgrown the idea that individuals can be classed simply as belonging to the auditory-motor, visual, mixed, *etc.* types, as regards their mental imagery. We know that a person may habitually make use of one kind of imagery, and yet be capable of extremely vivid imagery of another kind under certain circumstances: that is, we realize that the frequency with which a certain modality of imagery is used is not necessarily correlated with the intensity which it is capable of reaching in the mind of the same observer. In the same way, intensity and accuracy of imagery are not necessarily connected. An individual characteristic of great practical importance is the power to control visual imagery. Anyone who has to *think* in terms of visual space, the geometrician, the astronomer, the engineer, needs the power of shifting the parts of his visual images and making them take up new relations to each other, of moving them about in his mind without distorting the spatial relations which the actual objects would maintain if they were physically moved about. How far does ability to control visual imagery correlate itself with other characters such as frequency, intensity, and accuracy of visual imagery?

The present study attempts to study the correlation between ability to control the spatial relations of the parts of a visual image, with ability to reproduce accurately a complex visual image just seen. We shall refer to these characters briefly as 'control' and 'accuracy;' of course they are very special kinds of control and accuracy. Our measure of accuracy was obtained in the following way. We prepared a set of ten cards 8 cm. square. Each card was divided by ruled lines into four equal compartments, and in each compartment a different 'nonsense figure' of eight straight lines was drawn with red ink. Each of these cards was laid in turn on the table before the observer, who sat with closed eyes, and at a signal she opened her eyes and looked at the card for ten seconds. The card was then removed and the observer drew on a blank diagram as much as she could remember of the figures on the card. The same procedure was followed with the other nine cards. The problem of satisfactorily evaluating the results in terms of accuracy is not an easy one.

We proceeded on the general plan followed in Binet's letter-square method. We gave to a line correct in direction and in the right place on its design the value 1, and a line that was correct in direction but misplaced we counted as ½. A line added that was not on the design deducted ¼ from the total. An observer's total percentage of accuracy was found by adding together her 'marks' for all ten cards and dividing the sum by 320, the total number of lines on the ten cards. Of course a certain amount of liberty of judgment had to be allowed in estimating the value of a reproduction; in distinguishing, for instance, between a misplaced and an added line. The results of about half the observers were evaluated by one of the experimenters and those of the other half by the other experimenter: two separate estimates were made of all the results by the third author of the paper, and the evaluations were thus at least approximately uniform for the different observers.

Each observer whose accuracy in the immediate reproduction of a visual nonsense figure was thus measured was given, between every two accuracy tests, a test of the power to control the spatial relations of the parts of an image.

The plan of the control tests was the following. A square diagram of 3.5 cm. a side was drawn and divided by lines into sixteen equal compartments. In the uppermost and extreme left-hand compartment a small equilateral triangle was drawn. The observer looked at this diagram for five seconds; she then closed her eyes, and in pursuance of directions previously given, followed the experimenter's command to imagine the triangle moved to another square of the diagram. The experimenter directed ten such imaginary movements of the triangle, the movement to start each time from the position which the triangle occupied as the result of the last imagined movement. As the observer imagined a movement to be carried out, she signalled the fact to the experimenter, who then directed the next movement. When the ten movements had been imagined, the observer opened her eyes and indicated on what square of a blank diagram like the original the triangle would be placed as a result of the ten successive movements. A stop-watch had been started by the experimenter as she directed the first movement, and was stopped as soon as the observer reported that the last movement had been carried out. Ten such experiments were made with each observer, alternating with the ten accuracy tests. A different sequence of imaginary movements was of course required in each control test. The following sample series may serve as an illustration: "One square obliquely down and to right, two to right, two down, one to left, three up, one obliquely down to left, one to left, two down, two obliquely up to right, one to right;" "Two to right, one obliquely down to right, two obliquely down to left, two up, one to left, two obliquely down to right, one to right, two to left, one obliquely up to right, two up."

As measures of ability to control the image, there were thus available the total number of errors made in the final placing of the triangle in the ten experiments, and the average time required for carrying out the ten movements in a single experiment. Of course it might happen that a correct final placing of the triangle would result from a series of movements in which several errors were made that compensated for each other. The chances of this were not very great, however. The order of the observers as regards control was determined by multiplying the number of errors by the average time required. This value of course was inversely proportional to the degree of control. The few observers who made no errors in the ten control tests were put at the top of the list in the inverse order of their average times.

A comparison of the list of observers (forty-three in number, all young women) arranged in the order of the accuracy with which they reproduced the nonsense figures and a list arranged in the order of excellence of control reveals a complete absence of correlation between these two performances. The Pearson coefficient is +.073. The average per cent. of accuracy is 46.4, with a maximum of 66 and a minimum of 28. The average coefficient of control (number of errors multiplied by time in seconds) is 124.5, with a maximum of 286.2 and a minimum of 16.7. The observers fell into groups as follows. If they were divided as regards both accuracy and control into an upper division of fourteen, a lower division of fourteen, and a middle division of fifteen, the observers forming the largest group (eight) as regards correlation were those of moderate accuracy and moderate control. There were six who had high accuracy and high control, six who had low accuracy and low control, and six who had high accuracy and low control. There were only three who had low accuracy and high control. On the other hand there were five who had low accuracy and moderately good control. There were also five who had moderate accuracy and high control; two who had high accuracy and moderate control, and two who had low control and moderate accuracy. On the whole, then, it would appear that a person may be able to reproduce a nonsense figure in immediate memory with considerable accuracy, and yet be unable to move an imaginary triangle around a sixteen square diagram in ten successive positions without losing memory of the correct spatial relations, while on the other hand an observer may be very successful in the movement test and very poor at reproducing the nonsense figures. This latter possibility the originator of the problem (M. F. W.) confirms from her own general experience. She is a very poor visualizer, measured by the accuracy or vividness with which she can recall images of things seen, yet she has always been able to think clearly in spatial relations, and as an undergraduate had a special liking for geometry and trigonometry. There is a type of visual image, into which much kinaesthesis enters, which is stripped of color and details of form and represents spatial relations purely; these are all the clearer for the general bareness of the image. The psychology of this type of image deserves further investigation.

LABORATORY NOTES

By E. B. Titchener

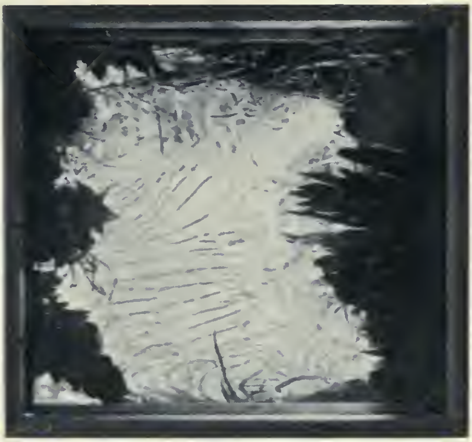
I. The Indian Harmonium

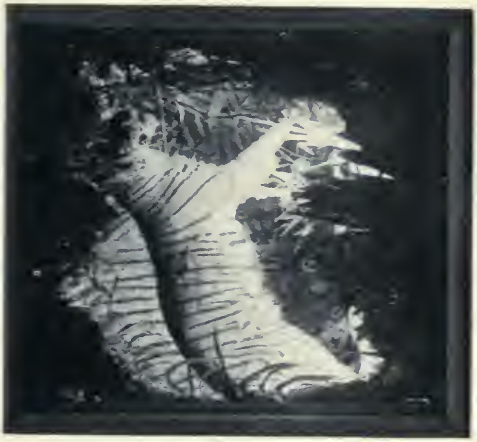
In view of the present interest in primitive and exotic music, it may be worth while to call attention to the Indian (Hindu) Harmonium manufactured by Messrs. Moore and Moore, New Oxford Street, London, at the price of twenty pounds. Style and finish are like those of the Ellis Harmonical, made by the same firm; instructions for tuning are furnished, so that the possible effect of the journey upon intonation need not be feared. The harmonium, designed with Mr. K. B. Deval's assistance by Mr. H. K. Moore, is fully described in a recent work by E. Clements,—Introduction to the Study of Indian Music, Longmans, Green & Co., 1913, $2.00 net. A more technical account is given in Specifications of Inventions, etc., 1911, lxxviii. (London, Patent Office, 1913), under no. 15548. Every octave has 23 notes, giving the 22 srutis or degrees of the Indian scale, with one additional note for modulation; the 11 extra notes are played by pressure upon studs which pierce the black and white manuals. The instrument is of great value both for class-room demonstration and for research.

II. A Thayer Tiger

Readers of A. H. and G. H. Thayer's *Concealing Coloration in the Animal Kingdom* (this Journal, xxi, 1910, 500 ff.) will remember the plate of the jaguar seen upon a background of forest leaves, and the photographs of the cardboard zebra against reeds and straws (pp. 132, 138), which illustrate typical obliterative picture-patterns of countershaded mammals. These plates are sufficiently striking, even though the animals appear in the flat, and even though the fixity of the camera-lens and the absence of color in the photographs make against obliteration.

The disappearance of the real animal into its background, under favorable conditions of illumination, is wonderfully shown by the model here pictured. The model, built by Mr. A. H. Thayer, is that of a tiger posed amidst long grass and seen against a grassy background. The two photographs were taken by the same camera, with no change of circumstance other than the shift of illumination. In the left-hand figure, the tiger stands out, solid and substantial, light and dark, just as it might be seen upon the floor of a menagerie cage; the lighting is due to three electric bulbs, two below and one to the right of the model. In the right-hand figure, in which the lighting simulates that of direct sunlight, the animal has lost solidity, has become amazingly transparent, and merges as a color-pattern into the background. An observer who views the change in an otherwise darkened room, at a distance of some 15 m., from a point squarely in front of the model, will literally lose the tiger for some ten seconds; with monocular vision, he will lose it for a longer time. No doubt, the sudden increase in the intensity of light aids this disappearance,—though it must be





remembered, on the other side, that the animal stays in place and that its contour is well-known when the change occurs. If the sunlight-effect is shown first, to an untrained observer, minutes may elapse before the tiger is made out; when it is made out, certain details (especially of neck and head) may be erroneously interpreted; and it is judged, when found, to be painted on glass and not modelled in the round, so that the assumption of solidity when the switch is turned comes with a distinct shock of surprise. I am more impressed, however, by the magnitude of the change when observed with full knowledge of lighting and construction.

The model has been set up permanently in the demonstrational laboratory of our Department of Psychology. It is the first model that Mr. Thayer has built for public exhibition. President Sanford and Professor Yerkes, who have seen it, join with me in the wish that its author—or some young artist of competence working under his direction—might be enabled to prepare similar models for the principal psychological and biological laboratories of the country.

III. The Holmgren Worsteds

I have examined a number of sets of the Holmgren Worsteds, as supplied by various dealers, and find that they differ very considerably. Holmgren himself (*Om Färgblindheten,* Upsala, 1877, p. 135) mentions as his authorized agent Frl. Letty Oldberg of Upsala. Frl. Oldberg still supplies the worsteds; and though they differ a little from the standards shown in Holmgren's colored plate, they agree more closely with these originals than does any other set that I have seen, while the differences are such as might be expected on the hypothesis that the colors of the plate are somewhat faded.[1]

The Holmgren test is so rough that the diversity of the sets found on the market probably makes very little difference in the results, provided that there is a sufficiently full series of skeins for comparison. For this reason I do not think it worth while to enter into particulars. Since, however, the test is still widely used, and since the educational importance of holding to a standard can, in general, hardly be overestimated, I have advised the C. H. Stoelting Co. to import the Oldberg series, and to supply it to all psychological and educational laboratories which order the "Holmgren Worsteds." Mr. Stoelting will furnish these authorized sets as soon as they can be procured.

IV. The Hering Grey Papers

The set of 50 Hering grey papers has not proved satisfactory.[2] Aside from the fact that many sheets are tinged with color (a fact to which I called attention in Experimental Psychology, I., ii., 1901,

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[1] The plate is reproduced in J. E. Jennings' *Color-Vision and Color-Blindness,* 1896, frontispiece, and in *Report of Royal Society Committee on Color-Vision,* 1892, facing p. 96. In the examples that I have seen, the reproductions differ both from each other and from the Holmgren original.

[2] I authorized the C. H. Stoelting Co., in 1902, to import these papers. I was not, as I need hardly say, responsible for the form of the advertisement in the firm's circular of that year. The advertisement was written with a too confiding trust in the statement of the German manufacturer.

32), the numbered series is often markedly uneven. The Leipzig manufacturer, Herr Mitter, sends the following information:

"Where the divergent shades occur, you have to do with a remanufacture of particular numbers. The reason is this: particular numbers are often ordered from the series, while the less used numbers remain in stock. Then, in order to make the series complete, the numbers that have been sold out are manufactured afresh. It is, unfortunately, impossible exactly to reproduce the original shades; a minute difference always appears; and though this difference may be hardly noticeable in itself [that is, I suppose, when the two papers, the new and the old are compared], it does show faintly in the series."

I have no doubt that this explanation is correct; and it is readily conceivable that, without a more careful process of manufacture than the demand for these papers and the price at which they sell now warrant, some unevenness is inevitable. I must add, however, that Herr Mitter seriously understates the defects of the series. His letter goes on to say that he has received no complaints from his German customers; and yet Hering himself wrote in 1907 (Lehre vom Lichtsinn, 83): "The series of grey papers prepared commercially at my suggestion unfortunately leave much to be desired."

The C. H. Stoelting Co. has gone carefully over its stock, regrading and renumbering; and Herr Mitter undertakes in future "die Differenzen auszugleichen, so gut es technisch möglich ist." But, when all is done, unevenness will remain; and it should be understood that a reference to "Hering grey no. so-and-so" indicates only approximately the grey actually employed. The papers are exceedingly useful; they cannot, however, be relied upon as standards.

V. The Hering Indirect-Vision Color-Mixer

Certain passages in Rand's *Factors that Influence the Sensitivity of the Retina to Color* (1913, 56) give the impression—so it seems to me and to other readers whom I have questioned—that the Hering Indirect-Vision Color-Mixer was designed not by Hering but by Hess. It is hardly likely that the writer fell into this mistake; but since some unwary student may be misled, I take this occasion to point out that the instrument has always been ascribed to Hering in Rothe's catalogues, and that Hess himself puts the matter beyond question (Arch. f. Ophthal., xxxv., 4, 1889, 25) by the remark: "Diesen Forderungen wurden in der folgenden Weise mit Hilfe eines *von Herrn Professor Hering angegebenen* Apparates erfüllt."

Rand asserts in a footnote that my Exp. Psych., I., ii., 1901, 20, "ascribes the description of this apparatus to Hering, giving as reference A. f. O., 1889, xxv., 4, p. 63." I do not, however, ascribe the description of the apparatus, but the apparatus itself, to Hering; and my reference is not xxv. but xxxv. In the article to which I refer Hering mentions the "nach neuen Methoden angestellten Versuche" of Hess, with a foot-note reference to Hess' own paper; and Hess (p. 2) speaks in like language of the "neuen von ihm [Hering] angegebenen Untersuchungsmethoden." The *Sachlage* is surely clear enough. Rand fails to state that I mention Hess on p. 26.

VI. The Hering Color-Blindness Apparatus

The original description of Hering's apparatus for testing color-blindness (figured in my Exp. Psych., I., ii., 1901, 7) speaks of "eine dem Apparate beigelegte Anweisung," a set of directions for the adjustment of the instrument in order to the performance of a test without waste of time. This *Anweisung* has been mentioned in various dealers' catalogues. There seems to be a reference to it in Spindler and Hoyer's large Catalogue XXI. (preface dated 1908). "Mit dem Apparate," we read, "wird eine Beschreibung nach Hering mitgeliefert, in welcher angegeben ist, welche Farben man zu wählen, und wie man bei der Herstellung der Gleichung zu verfahren hat" (p. 69). But this sentence may refer, on the other hand, simply to Hering's account excerpted from the Arch. f. Ophthal., which was formerly supplied by Rothe, and in which—to be sure—the apparatus is described, but which also contains the primary reference to the elusive *Anweisung*.

At any rate, I have never been able to secure the set of directions in question, although I have made enquiry of a number of colleagues, have written to Professor Hering himself, and have asked the principal dealers (Spindler and Hoyer included) whether copies were available. If any reader of this JOURNAL happens to possess the *Anweisung*, I shall be grateful for the loan of it, and will undertake, with the owner's permission, to reprint it. There is no particular difficulty in the use of the instrument, provided that "man sich auf den Standpunkt der Theorie der Gegenfarben stelle"; but uniformity of procedure is none the less desirable.

VII. DEMONSTRATIONS OF COLOR-BLIND VISION

Various attempts have been made to indicate by colored diagrams the appearance of familiar objects as seen by the partially color-blind. (1) The Report of the Royal Society's 1890 Committee on Color Vision, 1892, prints, facing p. 4, a plate of 5 spectra, normal, green-blind, red-blind, violet-blind, induced by disease. The plate is signed W. de W. A[bney]. (2) A similar plate, signed with the same initials and prepared by the same lithographer, but printed with right-left conversion, forms the frontispiece of W. de W. Abney's *Color Vision*, 1895. In the copy belonging to the Cornell University Library *blue* is entirely absent; I should say, from the general freshness of the plate, that, by an oversight, the blue pigment had never been applied. (3) The frontispiece of E. W. Scripture's *Thinking, Feeling, Doing*, 1895, shows the American flag as seen by most people, by red-blind persons, by green-blind persons, by violet-blind persons, by totally color-blind persons. In the edition of 1907 this plate has been suppressed, though a table on p. 136 tells how the flag appears to trichromats, dichromats I, dichromats II, green-blind, red-blind, blue-blind, and monochromats. (4) In W. F. Norris and C. H. Oliver, *System of Diseases of the Eye*, ii., 1897, W. Thomson and C. Weiland print four colored spectra (facing p. 315): that of the normal eye, a combined spectrum of potassium, lithium, sodium, thallium and caesium, that of the green-blind and that of the red-blind person. (5) In *The Century Magazine* for April, 1907 (lxxiii., 882 ff.) E. A. Ayers has colored plates of roses and leaves as seen by normal, red-blind and green-blind eyes; of three shades of red, green, violet as seen by three classes of color-blind; and of a Venetian scene as it appears to normal eyes and to a person partially color-blind in red. (6) There are colored plates of spectra, again, in Abney's *Researches in Color Vision*, 1913. Plate I (facing p. 275) shows, according to its legend, "spectrum colors as named by persons who were completely or nearly completely red or green blind," or shows, according to the text (p.

275), "how the completely red blind," for instance, "sees the spectrum."

All of these charts—and there are doubtless others that I do not know—are in one way or another misleading, whether the mistakes are due to theoretical bias, to the difficulties of color printing, or to sheer ignorance.[3] Yet it is distinctly of advantage to have demonstrations of the spectrum of the partially color-blind (ordinary red-green blindness, the blindness of the deuteranope or "green-blind," and the less common red-green blindness of the protanope or "red-blind"): the demonstration *ad oculos* is convincing, and saves the lecturer's time. I have therefore had two large spectra painted in oil by Mr. L. A. Fuertes; the canvases, about 150 by 20 cm., were first marked off roughly, at half-a-dozen points, for the principal colors, then subdivided for more accurate work into 35 or 40 strips of equal breadth, and then colored as quickly as possible from an already prepared palette. The yellow-series offers considerable technical difficulty; aside from questions of tint and chroma, it is exceedingly difficult to rule out every hint of green. General surroundings make more difference in this regard than even the trained eye would suspect,—so that a spectrum which appears greenless in the studio may show a faint wash of green when hung upon the wall of the lecture-room. This fact makes me hesitate to undertake the preparation of these spectra for others. Our own pair are excellent in our surroundings, and so long as Mr. Fuertes' services are available can be duplicated, in wide black frames, for about $25 apiece.

VIII. A Demonstration of "Tied Images"

I owe to Dr. W. S. Foster the suggestion that the pictures of Coles Phillips are useful for the demonstration of "tied" visual images. In particular, the young lady who is on her knees before a box of silver has been reproduced on a large scale for advertising purposes. Deprived of the silver (the unkindness is justified psychologically, because the box gives the outline of her knee), and pasted upon a dead-black background, she is an instructive figure; the observer "sees" a great deal that is not there to be seen; and the imaginal vision is, so far as our experience goes, practically uniform at certain points ("tied" imagery), and extremely variable at others.

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[3] C. L. Franklin, *Science*, xxv., 1907, 746.

A NOTE ON *SENSATION* AND *SENTIMENT*

By E. B. Titchener

In a recent number of the *Psychological Bulletin,* F. M. Urban has called the attention of experimental psychologists to Cournot's *Essai sur les fondements de nos connaissances.*[1] I am personally very grateful for the reference; Cournot's book is, for more than one reason, worth reading at the present day. In this Note, however, I am concerned only with a remark made by Urban. "The circumstance that Cournot uses the term 'sensation of pain,'" he writes, "where most writers of his period [the *Essai* bears date 1851] would have said feeling of pain, makes this passage sound quite modern."

The remark is a little ambiguous. It may mean that the phrases "sensation de douleur," "sensation de plaisir," are of recent coinage in French psychology, and that we might therefore have expected Cournot to continue the traditional use of *sentiment.* Or it may mean, again, that writers of the period of 1840-1860 had swung away from the word *sensation,* as applied to 'physical' pleasure and pain, and had adopted in its place the word *sentiment,* which has now once more, in contemporary French writers, given way to the older term. On the former interpretation, Cournot would be an innovator; on the latter, he might be considered either as reactionary or as more modern than the majority of his day and generation. I take it that Urban had in mind the traditional use of *sentiment.*

In fact, however, neither of these views appears to be correct. The phrases in question are very much older than Cournot, and their occurrence had been fairly continuous—in spite of all the vacillation of French terminology—for more than a century before he published; Cournot was not an innovator. Nor do I think that the year 1848, important as it was for French philosophy, marked any great change in this department of psychological terminology; the usage of Taine and of Rabier simply continues that of their predecessors. I base these conclusions, in the main, upon notes taken while I was preparing my lectures on *Feeling and Attention.* I have verified those references, and have here added a few more. The evidence, no doubt, is fragmentary; but it may be sufficient for the immediate purpose.

The following paragraphs would be more readable if I had worked up these notes into something like an essay on French terminology. Perhaps they may be useful, in other hands, as raw materials for a study that shall go behind the terms to the ideas which the terms are meant to convey. The French psychology which is here touched is, like the English psychology of association, terribly mixed up with theory of knowledge; and in the one case as in the other a confused terminology is the reflection of unclear thinking. An historical study would be instructive, if only for the parallel it affords to our own long struggle towards definite technical expression; but it would also, I believe, throw light upon that struggle, inasmuch as the French writers are, on the whole, more perspicuous than the English;[2] it is oftentimes surprising to find how distinctly the author's meaning stands out from a sheer medley of popular and technical terms.

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[1] An Historical Note, *Psych. Bull.*, xi., 1914, 24-26.

I hope, then, that some student of the history of psychology may be led, by the following notes, to attack the larger problem. This problem, be it remarked, is in no sense a problem of classification,—for which we might go to the indispensable Ueberweg-Heinze. The question is rather that of disentangling psychological fact from logical construction, observation from reasoning and interpretation; and it must be answered, not only from the works of the *idéologues*, but also from those of their spiritualistic and positivistic successors.

As to the notes themselves, I cannot be sure that I have always gone to the right books; some of the writers quoted are voluminous; but I have not, at any rate, made a prejudiced selection. I cannot be sure, either, that there is not already extant an essay or monograph that deals exhaustively with the subject; I can only say that I have not found anything of the sort; the historical chapters and articles that I have run across have been either very limited in scope or very sketchy.[3]

I

Descartes regularly employs the term *sentiment*.[4] Malebranche (1674-5) calls the perceptions of the senses *sentiments ou sensations*. We therefore find pleasure and pain numbered among the *sensations*, while we also find sensations of color, etc., termed *sentiments*.[5] Bossuet (1722), though he too often uses *sentiment* and *sensation* indifferently, remarks: "nous avons rangé le plaisir et la douleur avec les sensations."[6]

Buffon, in the *Histoire naturelle, générale et particulière* (1749-1804), declares that "la douleur et le plaisir sont des pures sensations." Yet he writes in another volume: "distinguons donc la sensation du sentiment: . . . l'essence du sentiment, son caractère unique est le plaisir ou la douleur."[7] Condillac, although he affirms in the *Traité des sensations* (1754) that pleasure-pain is inherent in the very nature of sensation, and therefore makes it his rule to speak of *plaisir, douleur* absolutely, nevertheless can write: "les sensations qui viennent par le tact . . . sont la chaleur et le froid, et différentes espèces de plaisirs et de douleurs."[8]

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[2] Or is this impression illusory, due to the relative unfamiliarity of a foreign language, and the restricted number of associations that its words and phrases arouse? Perhaps both factors are involved.

[3] I am indebted to my colleagues, Dr. W. S. Foster and Mr. H. G. Bishop, for following up various bibliographical trails. I have also to thank Dr. L. N. Wilson for the loan of a number of books from the Clark Library.

[4] *E. g., Passions de l'âme,* 1649, art. 94.

[5] N. Malebranche, *De la recherche de la vérité,* in *Oeuvres,* ed. J. Simon, iii, 1871, 30; 85, 89, 96, 98, 103, 346, etc.; 89, 93, 104, 114, etc.

[6] J. B. Bossuet, *De la connoissance de Dieu et de soi-même,* in *Oeuvres complètes,* v., 1846, 22, 53.

[7] G. L. L. de Buffon, *Oeuvres complètes,* nouvelle edition dirigée par M. Lamouroux, xvi. (Histoire des mammifères, i.), 1824, 57, 79, 81; cf. xiii. (Histoire de l'homme, i.), 1828, 317; and xviii. (Histoire des mammifères, iii.), 1824, 395.

Diderot's usage is vacillating, both in the *Éléments de physiologie* (written 1774-1780)[9] and in the *Encyclopédie* (1751-1772). The editorial article *Sensations* speaks of the *sentiment du plaisir et de la douleur,* and the author of the article *Sensibilité, Sentiment* is allowed to refer to pleasure and pain as *les deux sensations élémentaires.*[10] Condorcet, like Descartes, regularly employs the word *sentiment.*[11] Cabanis gives us a quotable passage: "les sensations de plaisir sont celles que la nature nous invite à chercher: elle nous invite également à fuir celles de la douleur."[12]

Bichat (1800) is the author of the famous phrase: "l'habitude émousse le sentiment," and we might expect him to be careful in his use of words. We find, in fact, the usual vacillation. "Le propre de l'habitude est d'émousser le sentiment, de ramener toujours le plaisir ou la douleur à l'indifférence": this statement accords with the definition of sentiment as one of the effects of sensation. "Le propre de l'habitude est d'agir en émoussant la vivacité du sentiment, de transformer en sensations indifférentes celles de plaisirs[13] ou de peine": here pleasure and pain are sensations; and Bichat speaks elsewhere of *une vive sensation de douleur.*[14] Pinel, too, writes of "les sensations de peine ou de plaisir."[15] And Tracy is emphatic on the point, as matter both of doctrine and of terminology, that pleasure and pain are sensations.[16]

Lamarck's general word is *sentiment,*—which, however, is very often made interchangeable with *sensation. Douleur* is expressly termed a sensation.[17] The discussion of pleasure leads us to the *sentiment intérieur* or *sentiment intime d'existence,* which plays a large part in Lamarck's physiological psychology. This *sentiment,* which is the resultant of a confused medley of organic sensations, may be thrown into action either indirectly, by the operations of intelligence, or directly, by a *besoin* which has itself been touched off by a sensation. Among the *besoins* of this 'physical' sort are, we are told, those "de fuir la douleur, de chercher le plaisir ou le bien-être." But another passage informs us that the *besoins physiques* are "ceux de se soustraire à la douleur, au malaise, de satisfaire à la faim, à la soif, etc.," while the *beoins moraux* are "tels que ceux de chercher le plaisir, le bien-être, . . . etc." What the *plaisir ou bien-être* actually is, does not appear; probably it is of two kinds, physical and moral, sensation and idea.[18]

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[8] E. B. de Condillac, *Oeuvres complètes,* iii., 1798, 58, 243. Characteristic phrases occur also 40, 76, 96, 122 f., 174 f., 205.

[9] D. Diderot, *Oeuvres complètes,* ix., 1875, 311, 356, 439; ct. 374, 381.

[10] *Encyclopédie ou dictionnaire raisonné des sciences,* etc., xxx., 1780, 735, 741.

[11] M. J. A. N. C. de Condorcet, *Esquisse d'un tableau historique des progrès de l'esprit humain* (written 1794-5), in *Oeuvres complètes,* ed. by A. C. O'Connor and F. Arago, vi., 1847, 340, 544.

[12] P. J. G. Cabanis, *Rapports du physique et du moral de l'homme,* 1802; Paris, 1824, i., 192. I am inclined to think that this passage is unique: it is, however, not an afterthought, as it occurs in *Mémoires de l'Institut National, Sciences Morales et Politiques,* I., Thermidor An VI., 207. The nearest approach to it that I have found is i, 1824, 180; ct. iii., 179. *Impression* seems to be used more or less indifferently for excitation and mental process (*e. g.,* i., 190; ii., 276); *sentiment* is very common; phrases like *sensations voluptueuses, agréables* (ii., 231, 276) are also frequent.

[13] The plural is probably due to a printer's error.

[14] M. F. X. Bichat, *Recherches physiologiques sur la vie et la mort,* ed. by F. Magendie, 1829, 57, 115, 116. Magendie himself writes (58): "le plaisir et la douleur sont toujours des sensations absolues."

[15] P. Pinel, *Traité médico-philosophique sur l'aliénation mentale ou la manie,* An IX. (1800-1), xxiv.; the word *sentiment* is also used.—The *Encyc. Brit.* dates this work 1791.

[16] A. L. C. Destutt de Tracy, *Élémens d'idéologie: idéologie proprement dite,* (1801) 1826. For terminology cf. 19, 26; for doctrine, 25, 26 f., 27 f.

II

In sharp contrast with Tracy stands Laromiguière (1815-18), who combats the indiscriminate use of *sensation,* and professes himself to write with rigorous precision. His fundamental term is *sentiment,* of which he notes four distinct kinds: the sensation-feeling or sensation, the feeling of the action or operation of the mind's own faculties, the feeling of relation, and the moral feeling.[19] Chronologically, sensation is the first thing in mind: "la sensation, c'est la première manière de sentir que nous remarquons en nous." Logically, however, sensation is "une idée complexe;" "l'effet immédiat de l'impression que les objets font sur nous" is the *sentiment;* "la sensation consiste dans le sentiment rapporté aux organes du corps, ou aux objets extérieurs;" "la sensation a son origine dans le sentiment; et on peut la définir, un sentiment jugé ou rapporté hors de l'âme."[20] Further, sensation may be taken in two ways: specifically, as color, tone, and so forth; and generically, as affecting the mind for good or ill (*plaisir ou douleur*) and as informing or reminding it of its own existence (*sentiment du moi*).[21]

But this distinction offers, of course, an admirable opportunity for the sort of terminological confusion that we have found elsewhere. Laromiguière does not fail to take advantage of it. Now it is the *sentiment,* now the *sensation,* that affects the mind;[22] pleasure and pain are *sentiments,* while yet there are painful or indifferent *sensations;*[23] the pleasure that we gain from a beautiful sight is a *sentiment-sensation,* that is, a sensation proper.[24] The time is not yet ripe for standardisation, and for all the author's efforts his language goes the accustomed way.

Next on my list is the *Physiologie du goût* (1825). I have no wish to quote this celebrated work as an authority on technical terms;

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[17] J. B. P. A. de Monet de Lamarck, *Philosophie zoologique,* etc., (1809) 1873, i., 6, 9 f., 26; ii., 155, 166, 171, 184, 235, 245, 306.

[18] ii., 257 f., 260, 297, 305, 357.

[19] P. Laromiguière, *Leçons de philosophie,* etc., 1844, i., 159; ii., 56, etc.

[20] ii., 44 f.; i., 160, 316.

[21] i., 71; ii., 43 f., 131.

[22] ii., 47, 131.

[23] ii., 140; i., 71, 160.

[24] ii., 367.

but I suppose that it reflects the terminology of its day, and I suppose that Cournot—like everybody else—had read it. If that is the case, he found pleasure and pain called sensations in Meditations 14, 19, 26.[25]

I turn to the physiologists by profession. Blaud in 1830 speaks freely and often of sensations of pleasure and pain;[26] so does Dufour in 1833.[27] For Longet (1850) pleasure and pain are sensations of the "sensibilité générale;" the phrase 'sensation of pain' occurs; I have not chanced on 'sensation of pleasure,' although appetite, the first and agreeable state of hunger, is called sensation.[28] In the section on Hunger an effort is made to distinguish between the localised *sensation* and the general *sentiment;* but the distinction is not carried through; and presently we read that "la faim parle plus haut que les lois, que la raison, que les sentiments!"[29] Béraud (1853) goes into greater detail. Every sensation, general or special, internal or external, may appear as "indifférente, agréable ou pénible."[30] *Douleur* is not a sensation, but a degree of sensation; every sensation may pass over into pain; pleasure-pain depends, in fact, upon the state of the organ or tissue, so that, for instance, "le besoin d'uriner, . . . comme toute sensation interne, est *plaisir* ou *peine,* selon qu'on cède ou qu'on résiste a son voeu."[31] Yet we meet, it need hardly be said, with the phrase "sensations de douleur."[32] As for the *sentiments,* they are at first internal sensations, and include needs, desires, passions, satisfactions, as well as hunger, thirst, etc. Later in the work the "*sentiments* proprement dites" have become affective processes, "émotions dites d'affection et de repulsion, de plaisir ou de déplaisir;" "la répétition émousse le sentiment, c'est-à-dire les émotions qu'elles font naître."[33]

Béclard (1855), in his section on Touch, speaks without hesitation of the sensation of pain.[34] Touch and pain do not, indeed, belong to different senses; they are only different modes of expression, or different degrees, of the same sensibility.[35] As for pleasure, "les sensations voluptueuses du tact . . . constituent, en quelque sorte, le pendant de la douleur." Here is modernity with a vengeance! We are reminded of Stumpf's theory of affective sensations, and we have an anticipation (since the "sensations voluptueuses" are assimilated to tickling) of the theory of Bourdon.[36] But touch-pain and touch-pleasure do not account for the agreeableness and disagreeableness of hunger and thirst, smell and taste. Man is endowed, Béclard tells us, not only with intellectual faculties based on sensations, but also with affective faculties whose point of departure can be traced to organic needs. "Les instincts sont des besoins plus ou moins impérieux, qui ont pour sanction le plaisir et la douleur;" and these instincts, as motives to reasoned action, as 'perceived' by man and not left to act blindly, become *sentiments.* The position is not further worked out.[37]

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[25] A. Brillat-Savarin, *Physiologie du goût,* nouvelle édition revue avec soin, Paris. n. d., 175, 216, 218, 259, 265.

[26] P. Blaud, *Traité élémentaire de physiologie philosophique,* 1830, i., 178, 332; ii., 76, 79, 80, etc.

[27] P. Dufour, *Essai sur l'étude de l'homme,* i., 1833, 46, 63 f., 330.

[28] F. A. Longet, *Traité de physiologie,* 1873, i., 23; iii., 149; for the general position, see iii., 585, 591.

[29] i., 24.

[30] B. J. Béraud, *Éléments* [*Manuel*] *de physiologie de l'homme,* etc., i., 1856-7, 143.

[31] i., 143, 153, 155, 161.

[32] ii., 772.

[33] i., 144; ii., 827, 829 f.; cf. 619.

[34] J. Béclard, *Traité élémentaire de physiologie humaine,* 1862, 882, 884, 893; ct. 887.

[35] Touch and pain, 884 f.; temperature and pain, 893.

[36] 896: cf. my *Feeling and Attention,* 1908, 81 ff., 338.

With these physiological books we have overshot the year 1851, the date of Cournot's *Essai;* but I wished to show that the use of *sensation* in regard to pleasure and pain is continuous. I turn back now to the philosophers. In his chapter on the *vie animale* Comte writes (1838): "ce sont les sensations intérieures qui se rapportent à la satisfaction des divers besoins essentiels soit de nutrition, soit de reproduction, et auxquelles il faut joindre, dans l'état pathologique, les différentes douleurs produites par une altération quelconque."[38] Pleasures and pains are here sensations, though the following essay, on the intellectual and moral functions, speaks the language of Gall. Maine de Biran, in an essay which appeared for the first time in 1841, declares: "partout où est la vie là est aussi quelque degré de sensation affective de plaisir ou de douleur."[39] Jouffroy, again, furnishes us with passages like this (1845): "l'homme est-il empêché dans son developpement intellectuel et productif, il éprouve une sensation désagréable. Est-il au contraire aidé, il éprouve une sensation agréable. Etre contrarié et impuissant, voilà pour lui la peine. Etre secouru et puissant, voilà pour lui le plaisir."[40]

III

Bouillier (1865), like Laromiguière, makes a great profession of accuracy, and even devotes a chapter to the historical "équivoques du mot Sensation." *Sensibilité* is defined as the faculty of experiencing pleasure and pain.[41] The manifestations of this faculty are *sensations:* sensation is set off, as a "phénomène purement affectif," from the *perceptions des sens.* So we have "sensation de douleur," "sensation de bien-être ou de malaise," etc.[42] That is all in order; but Bouillier's usage is otherwise disappointing. *Sensation* and *sentiment* are, from the very first, run together, and where they are distinguished the difference between them is left to the reader to supply.[43]

Taine (1870) writes that "les nerfs des muscles, comme ceux de la peau, peuvent donner naissance aus sensations de contact, de froid et de chaud, de plaisir et de douleur."[44] And Rabier (1884) distinguishes "les sensations de l'ordre affectif, ou plaisirs et douleurs sensibles, et les sensations de l'ordre représentatif."[45]

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[37] 20 ff., 22 f., 869, 877, 1022, 1024 f.

[38] A. Comte, *Cours de philosophie positive,* iii., 1838, 741, 806.

[39] F. P. G. Maine de Biran, *Oeuvres philosophiques* publiées par V. Cousin, iii., 1841, 218. The essay, whch contains a long discussion of coenaesthesia, was apparently written in 1813 and retouched in 1823. Cousin himself, in his popular work *Du vrai, du beau et du bien,* (1836) 1881, 136, etc. speaks of *la sensation de l'agréable.*

[40] T. Jouffroy, *Cours d'esthétique,* 1845, 12 f., 38 f., 40, 268.

[41] F. Bouillier, *Du plaisir et de la douleur,* 1885, 8, 41.

[42] 26 f., 50, 309.

[43] ix. f., 23, 176, 300, 306 f., 338.

So we have come—though not by a series of minimal changes—from Malebranche down to the present day. It remains only to point out that Cournot's *Essai* has itself no settled terminology. In the passages quoted by Urban, Cournot uses "sensation de plaisir," "sensation de douleur," as well as "sensation douleureuse ou voluptueuse;" he also speaks of the "sentiment intime d'effort musculaire;" and elsewhere again he identifies sensation with "affection sensorielle," and makes the ear "la siège d'affections agréables ou désagréables."[46] Both the forms of expression and their variation reflect, I believe, the usage of his day.

If, then, my evidence can be regarded as adequate, the conclusion of this Note is that, during a period which extends from the generation next after Descartes to the present time, the word *sensation* has been used freely and naturally, by French philosophers and physiologists, to denote pleasure and pain.

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[44] H. Taine, *De l'intelligence,* i., 1883, 218.

[45] E. Rabier, *Leçons de philosophie: Psychologie,* 1896, 93. Rabier furnishes the basis for J. M. Baldwin's treatment in *Handbook of Psychology: Senses and Intellect,* 1890, 82 ff.

[46] A. A. Cournot, *Essai sur les fondements de nos connaissances,* etc., i., 1851, 186 f., 199, 215, 227, 396 f.

BOOK REVIEWS

Qu'est-ce que le raisonnement? Deuxième Partie. De l'intuition à la déduction. PAR EUGENIO RIGNANO. Scientia. XIV., 1913, pp. 129-156.

The author having in a previous article discussed the simpler forms of reasoning, now passes to a second aspect of its evolution, namely, intuition and deduction. The former term he defines as "any new assertion which arises spontaneously and unexpectedly, *i. e.,* which occurs without previous search or intentional observation and without being preceded by repeated trials of pre-established verification." It may be the discovery of an attribute, of an object, or of an analogy between phenomena which occur suddenly, with no change in external conditions, but with the awakening of an affective state which, accidentally coinciding with these conditions, makes them of interest for the first time. This is practically what happens in every act of perception. Among the numerous sensible qualities, we discover those which affect this or that affectivity, and the unforeseen discovery of a new attribute in a familiar object is only a complement of the previous perception of the object in consequence of a new affective point of view. Galileo, for example, discovered the law of the isochronism of the pendulum by an intuition due to his mental preoccupation of an affective nature relative to the measure of time. The same result might have been reached by an observer who set himself to observe how the duration of the oscillations gradually diminished in proportion to the amplitude. In such a case, an affective preoccupation of the same sort as in the former case would have been implied, but the result would have occurred with certainty and not by chance. The nature of the discovery however would not have differed from that made by intuition.

Axioms and postulates, the empirical character of which is no longer disputed, are discovered by intuition aroused always by an interest, *i. e.,* by an affective state. As an example of the discovery of an axiom by intuition in animals the following case is quoted from Romanes. "A dog following the trail of his master along a street came to a point from which three streets led in divergent directions. Sniffing along two of the streets and finding the trail in neither of them he immediately ran down the third street without stopping to sniff." Here the desire of finding his master led him to the discovery of the axiom that if there are only three alternatives and two are excluded the third must be correct.

The first intuition of all the postulates of transition, among them the postulate that through a given point only one parallel can be drawn, is probably due to the discovery that the passage from one class of equivalent phenomena to another class of phenomena equally equivalent but opposed to the other from the affective point of view, is by way of the unique phenomenon of transition, which belongs to neither class.

From intuition as a mere statement of some fact or attribute which is immediately perceived we pass by degrees to intuition as discovery which comes after a mental combination of experiences, often very simple, which are merely thought and this combination occurs suddenly and spontaneously under the impulse of a unique affective activity without control or correction by a state of corresponding attention, *i. e.,* there is present no secondary affective state which exercises an inhibitive control. It is probably to this kind of intuition, as has been shown in Part I of this discussion, that most of the thought experiences of animals belong.

But for another combination of mental experiences, otherwise completely analogous to the preceding, a certain amount of reflection is necessary. This may be due either to the fact that at first no mental combination of any kind presents itself, or it may be that the first combinations show themselves unsuitable for attaining the end sought. But the sole difference between this form of reasoning and intuition consists in the fact that, in the one case, the correct combination is attained only by repeated trials of which several must be rejected before one is chosen, while in the other, the "happy idea" accidentally presents itself first and immediately becomes the one chosen. But it is precisely by means of separate trials that reflect-reasoning excludes in great measure the chance attainment of the result which takes place in intuition.

Intuition also leads us to the discovery of the general validity of a particular demonstration, as in geometrical demonstrations. This immediate vision consists in the fact of seeing mentally and instantaneously that when the same series of experiences which make up the demonstration are repeated upon imaginary figures, of which certain aspects are varied, the result always remains the same. Intuition in this case then resolves itself into the mental repetition of as many of the series of experiences as there are different forms of the figure mentally presented, and each of these series is similar to the first one carried out upon the actual figure drawn on the paper or blackboard. But generalization does not always occur through intuition. Though not at all changed in its nature it frequently exacts a certain amount of reflection which sooner or later succeeds in selecting the right combination, when intuition, unless favored by chance, might fail.

Sometimes intuition consists in the finding of analogies between phenomena which have previously been considered as totally different when viewed from the point of view of perception, but which nevertheless have some common attribute and are therefore equivalent as regards the obtaining of a particular result. The designation of this particular attribute, necessary and sufficient for such an equivalence, constitutes a scientific law. This extension of all that is known of given phenomena to other unknown phenomena is the foundation of induction and it is also this, as will be shown later, which permits the application of deduction in science. It differs from the generalization of a particular demonstration in that the result to be obtained is not already fixed in advance. As a result of this difference, intuition, which here is the insight of genius, cannot be entirely replaced by reflection. The principle of continuity, *i. e.,* the tendency to extend certain properties of a phenomenon which have been verified only under given circumstances, is precisely the same principle which the geometrician follows when he imagines the triangle under his eyes varied in every possible way. But all new analogies cannot be discovered by the principle of continuity, and it is just here that the intuition of the man of genius is of supreme importance. There is thus an absolute necessity in science of continual alternation of intuition and reflection, the former to avoid sterility, the latter to control and test the validity of each new discovery.

More complicated forms of reasoning which are less susceptible of attainment by intuition and which most frequently require a certain amount of reflection are the principle of sufficient reason, demonstration by *reductio ad absurdum,* and the syllogysm. As an example of the first two the author gives the mental process by which Stevin arrived at the formulation of the laws of equilibrium along an inclined plane. He imagined an endless chain placed over a triangular prism with two faces oblique and the third horizontal. The free part of the chain being in equilibrium could have no effect either to cause or hinder motion in the parts resting upon the inclined planes. This statement is in accord with Liebling's law of sufficient reason and depends upon the fact that the discovery that symmetry by itself implies the possibility of carrying out mentally two identical experiments upon the object or phenomenon and obtaining from these two identical experiments an identical result. This may be accomplished either by means of a simple change made between the symmetrical elements or by a change in the attitudes of the observer. Finally there is a mental verification of the result. Thus far the series of operations is not so complex but that they might be reached by simple intuition, but in the mental process followed by Stevin is found also an application of *reductio ad absurdum;* for the next step in the reasoning process is that apart from friction, motion, once initiated into such a chain, would be perpetual, since change in the equilibrium would be impossible. This is the regular procedure in *reductio ad absurdum,* the non-possibility of a fact not being easily established. An impossibility easier to recognize is established, which a mental combination more or less complex shows to be the inevitable consequence of the first. It depends then upon the principle of contradiction, *i. e.,* upon the reciprocal inhibition of antagonistic images, a mental process which the author considers analogous to retinal rivalry. The affective state, the particular *malaise* which is characteristic of a logical contradiction is the same as in retinal rivalry. In both cases the disagreeable conflict provokes the corresponding desire of a reconciliation and ends by the elimination of one of the contradictory images. This may occur either because one is the product of the imagination or the antagonistic images may prove by reference to material facts to be two distinct objects. For instance, if one large and one small ball are attached to the arms of a balance and the latter, contrary to expectations based on experience, depresses the arm to which it is attached, a contradiction arises, which however disappears when it is found that the larger ball is hollow and therefore not equivalent to the mental image which was contradictory. It is further necessary that two contradictory conceptions in order to be mentally exclusive must be active at the same time, otherwise the contradiction is not observed, in the case of mental rivalry does not, in fact, exist. This explains the fact that individuals may often accept two contradictory propositions. Reasoning by exclusion is only a form of the principle of elimination of contradictory ideas.

The syllogism in the classic form consists in mentally separating, under the influence of an affective impulsion, the attribute which is necessary and sufficient to render several objects equivalent by agreement with the tendency. It is mentally directed perception, *i. e.*, "a complement of perception in a determined direction, obtained by putting the observer or thinker at that point of view in regard to the end which is set forth by a general proposition." Or it may be a mental perception taking place at different times, so that interest is transfused from one attribute to another, and its importance lies in precisely this transfer of interest. It also permits a saving of time as regards induction and by the generalization contained in the major premise can lead to the discovery of new truths, that is, may be really productive.

The last section of the present article deals with causal explanation and deduction. The mind seeks inevitably to discover the how and why of every phenomenon and by examination of the process we find it to consist in a chain of experiences simply thought, which, by means of phenomena already known, allows us to arrive at the phenomenon to be explained. It is the necessity of inferring all phenomena, even those of the most complicated nature, from a small number of elementary phenomena which are known to us which constitutes what we call the necessity of a causal explanation, and it is from this necessity that the tendency of science to become more and more deductive is derived, but this tendency is quite a different thing from the possibility of its so becoming. It would be quite impossible to accomplish the vast number of combinations required by mental representation only, because it would be necessary to know the result of each of these separately. But if on the contrary, by means of intuition and the corresponding more or less hypothetical generalizations, those combinations which are equivalent as shown by agreement with a given result which has been experimentally verified, can be discovered, then the result can be applied to other combinations which are simply thought. Every extension of the deductive method implies a new corresponding induction and by this means the previously unknown results of a whole category of experimental combinations become known because assimilated with other categories of experimental combinations whose validity has been established. It is thus that we arrive at the discovery of concepts, laws and formulas which express new inductive generalizations. This generalized or schematized order permits the representation of whole groups instead of individual cases and thus by reason of the greater simplicity, permits a more extended series of mental processes. Thus general and abstract concepts contribute largely to facilitate the more extensive use of the deductive method, and thus the formation of concepts and the application of the deductive method proceed *pari passu*. It is evident that so complicated a series of mental acts could never be carried out by intuition alone but that intense and prolonged reflection, in which the primary affective activity, held in check by the secondary affective activity, is necessary in order to follow out and test all the mental combinations, to reject the unsuitable and to combine the experiences successively thought into a unity. But thought experiences alone would also be insufficient to arrive at conclusions unless tested by comparison with material facts and this is facilitated by means of a schematic representation of graphic symbols.

The more complex forms of reasoning in the exact sciences the author leaves for discussion in a later contribution.

THEODATE L. SMITH.

BOOK NOTES

The socialized conscience. By JOSEPH HERSCHEL COFFIN. Baltimore, Warwick & York, 1913. 247 p.

This book is devoted to "our newly created moral problems." Its purpose is to suggest in present-day psychological and sociological terms a working hypothesis, a moral criterion, by means of which the different types of moral situations may be met with some degree of constancy. The topics of the chapters are as follows, moral control, the moral situation, personality and the moral criterion, the criterion applied, the home, to the school, and other cultural agencies, to vocation, the state and the church, the moral ideal.

An elementary psychology; suggestions for the interpretation of human life. By D. E. PHILLIPS. Boston, Ginn (c. 1913). 352 p.

In this volume the author simply seeks to arouse a deeper interest in a general science of great practical importance to every individual, and his book is based on some twenty years of experience in starting students in this field. The author's work has been very successful and his book is well worth writing and approaches the subject in a new and effective way. He begins with the fountains of human conduct,—the will to live and instinct as the fountain of conduct, imitation as a fountain of conduct. Under the latter he discusses habit, the feelings and their development, and apperception, then passes to the relations of psychology and evolution, discusses the nervous system, its functions and education, sensation and the development of the senses, relation and association of ideas, development of memory and imagination, problems of heredity and environment, the thinking process and its development, suggestion and mental healing, social psychology, will, freedom and education, magic and spiritism, psychology in literature, music and art, and ends with some general reflections on human conduct.

Harvard psychological studies. Volume 3. Edited by Hugo Münsterberg. Cambridge, Harvard University Press, 1913. v. p.

The first two volumes of the Harvard Psychological Studies, we are reminded, contained 1,300 pages of reprints on researches in the Harvard psychological laboratory, and appeared as independent books in the market. Then this method seemed undesirable because "such large books from one single laboratory could not possibly find a circulation as wide as that of the general psychological magazines." Moreover, the work covered such a wide range of subjects that it required that any who wanted one should purchase the whole. There was great delay, too, because the manuscripts had to lay over until they totaled 600 pages. Then the publications occurred in isolated monographs, and "our publications lost the effect of unity," hence the Harvard laboratory will keep on publishing in any journal with quick, wide distribution, but will collect and publish reprints by the volume-full for the market, to be distributed through laboratories, among libraries, etc. Each volume thus published, of which this is the first, will contain nothing not printed elsewhere first, and what is here contained are reprints, just about evenly divided between animal psychology, under the supervision of Professor Yerkes, and human psychology under the supervision of Professors Münsterberg, Holt and Langfeld. Under human, eight, under animal psychology, nine, monographs appear. The fact that the articles are published in such various periodicals accounts for the unusual disparity of size of type, length of line, etc.

Religio Doctoris; meditations upon life and thought by a retired college president. With an introduction by G. Stanley Hall. Boston, Richard G. Badger (1913). 183 p.

This book has a peculiar interest and charm because it was written by a man of culture who lived for some time in the expectation of speedy death, and who sought to fortify his soul by forming his personal convictions in non-technical terms concerning the supreme problems of human life. Being a special student of philosophy, and under the above conditions, he was able to express himself with a candor and frankness unalloyed by all prudential considerations. Since his partial convalescence, urged by his friends, he decided to publish his reflections, but has done so without revealing his identity. The first chapter is a revery, telling how he came to conceive these essays. The second points out the relations between philosophy and every-day life, the third, what explanation and the true interpretation of the principle of cause and effect means; and then comes the problem of evil, and finally, happiness and morality.

Marias Jungfräuliche Mutterschaft; ein völkerpsychologisches Fragment über Sexualsymbolik. Von A. J. STORFER. Berlin, Hermann Barsdorf Verlag, 1914. 204 p.

This book is an attempt to apply so-called Freudian principles to Maryolatry and Christolatry generally. The previous life of the holy mother, the annunciation, the extended arms in one ancient picture, the legend of the stairway, of weaving, of unbound hair, the maidens' consecration to gods in ancient temples, veils, lilies, myrtle,—all these in the mythic traditions of the holy mother are given a more or less phallic interpretation. The author then proceeds to consider Joseph, his staff and rod, his wooing and victory, which saga has amplified, the myth of the serpent, the holy word, tongue, myth, breath, ray, rain, wings, sword, branch, sceptre, unicorn, mill, earth, paradise, fountain, source, vessel, city, fortification, temple, bridal chamber, door, gate window; and finally, the few main incidents in the life of Jesus, especially the death and resurrection,—all these are given a gross sex interpretation, apparently without the slightest justification, so that the book gives, as the writer can testify, to more than one person, the impression of being simply a morbid and repulsive, not to say sacreligious, attempt to give double meanings which suggests the smut talk of the street gamin. Symbolism can perhaps be no madder than here.

Heilen und Bilden; ärztlich-pädagogische Arbeiten des Vereins für Individualpsychologie. Hrsg. von ALFRED ADLER und CARL FURTMÜLLER. München, Ernst Reinhardt, 1914. 399 p.

This association of doctors and pedagogues for the study of individual psychology has done well to print here a collection of nearly thirty of their essays and papers. The topics included are such as the doctor as educator; the theory of *Minderwertigkeit* and its significance for psychology; the aggressive impulse; educational errors; neurotic disposition; obedience and obstinacy; psychic hermaphroditism; education of parents; organ dialect; nervous character; impulse of activity and nervousness; strictness in education; psychology as a practical calling; choice of a profession; children's suicides, etc.

Wort und Seele; eine Untersuchung über die Gesetze in der Dichtung. Von HELLMUTH FALKENFELD. Leipzig, Felix Meiner Verlag, 1913. 132 p.

This little book treats in successive chapters, poetry and allied arts, tragedy of dilettantism, the soul and the law of value, word and anger in the drama, love, the soul of the world, humor and grotesqueness.

Nature and cognition of space and time. By JOHNSTON ESTEP WALTER. West Newton, Pa., Johnston & Penney (1914). 186 p.

This is a very philosophical discussion upon reality and nature of space, our conceptions of it, localization of tactile sensations, nature and cognition of time, its reality in nature, our conception of it.

The philosophy of faith; an enquiry. By BERTRAM BREWSTER. London, Longmans, Green and Co., 1913. 201 p.

The chapters here are entitled truth, virtue, freedom, optimism, beauty, the highest good.

Recent developments in China. Clark University addresses, November, 1912. Edited by George H. Blakeslee. N. Y., G. E. Stechert & Co., 1913. 413 p.

This volume contains twenty-two addresses, all of them by experts, that were given at the Clark University Conference on China in November, 1912. The purpose of this conference was to diffuse among the American people a better knowledge of present conditions and problems in the celestial empire. The missionary interests, though not excluded, were very subordinate. This conference is one of a series which has been held for a number of years for the same purpose, the first being on Japan and the last upon South, or rather Spanish America, with one session upon problems of the near east.

Probleme der Entwicklung des Geistes. Die Geistesformen. Von SEMI MEYER. Leipzig, J. A. Barth, 1913. 429 p.

The author first considers the impelling forces in the development of the soul, its manifoldness, then proceeds to the discussion of the functions and primitive forms of consciousness, the nature of sensation, the problem of feelings, instincts, motivation, memory, the origin of the will, the evolution of movements, practice and skill, mechanization, complementation and *Verdichtung,* things, space, time, with a final summary on the development of consciousness. This book appears to be a first *Lieferung* of a larger series devoted to problems of development.

Die geistige Kultur der Naturvölker. Von K. Th. Preuss. Leipzig, B. G. Teubner, 1914. 112 p.

This is a condensed statement of the life and thought of primitive people, their magic, deities, religion, science and art, set forth in a way to indicate that we have not given them sufficient credit for culture; that before the principle of division of labor entered and everybody came nearer doing and knowing everything, individual culture was much higher than we have hitherto supposed.

Totem und Tabu; einige Übereinstimmungen im Seelenleben der Wilden und der Neurotiker. Von Sigm. Freud. Leipzig, Hugo Heller & Cie, 1913. 149 p.

Freud has here set forth in considerable detail his own conception of totem and tabu, illustrating at great length his view that the psychoses of neurotics and of primitive people are very similar. Whatever one may think of these conclusions, the stimulating and suggestive quality of this work and indeed of all Freud writes, stands out very prominently.

Papers on psycho-analysis. By Ernest Jones. New York, William Wood & Co., 1913. 432 p.

It is very convenient to have the many scattered and very able papers of this brilliant Freudian accessible between two covers. The papers are first, general, treating everyday life and Freud's psychology in general. Then come clinical papers on amnesia, modern conceptions of psychoneuroses, relations between organic and functional disease, simulated foolishness and hysteria, the psychology of morbid anxiety. In part three we find papers on treatment, psychoanalysis and psychotherapy, psychoanalytic method generally, the word association method, the action of suggestions, reflections on some criticisms of the psychoanalytic method of treatment, therapeutic action of psychoanalysis. The fourth group has papers on dreams, first the general discussion of Freud's theory, then the influence of dreams on waking life, the relations between dreams and psychoneurotic symptoms, a forgotten dream; and finally come two papers on education, its relations to psychoanalysis and the value of the subconscious processes for education and re-education.

Audition and habit formation in the dog. By Harry Miles Johnson. Behavior Monographs, vol. 2, No. 3, 1913. Cambridge, Holt, 1913. 78 p.

These experiments show that it is impracticable to attack the "Molyneux problem" by using dogs rendered temporarily blind by this operation. Vision is not necessary to enable the dog to make quite complex adjustments. The dog's behavior indicates that he makes little use of vision but relies mostly on kinaesthetic and muscular sensations. The rate and method of learning in blind and normal dogs shows very little difference. Dogs given only ten trials a day required fewer trials for learning on the average than those given twenty trials, suggesting what is the optimal number of daily trials that tend to produce perfect habits with least effort.

Diseases of the nervous system. By ALFRED GORDON. 2d ed. rev. and enl. Philadelphia, P. Blakiston's Son & Co., 1913. 618 p.

In this second edition nearly every chapter has been enlarged, but the chief additions are on skull fracture, congestion of the brain, lumbar puncture, cerebro-spinal fluid, Wassermann's reaction, and psychoanalysis.

Les maladies sociales. Par PAUL GAULTIER. Paris, Librairie Hachette & Cie, 1913. 270 p.

The first chapter discusses adolescent criminality; the second, alcoholism, and then follow chapters on depopulation, pornography, and suicide.

Life, emotion and intellect. By CYRIL BRUYN ANDREWS. London, T. Fisher Unwin (1913). 95 p.

After an introduction on life and psychology, the author considers our attempts to suppress emotion, love and friendship, religion, the stage, law and crime, struggle and growth.

Are the intensity differences of sensation quantitative? IV. By WILLIAM BROWN. (Reprinted from the British Journal of Psychology, Vol. VI, Part 2, October 1913). pp. 184-189. Cambridge, University Press.

The effects of 'observational errors' and other factors upon correlation coefficients in psychology. By WILLIAM BROWN. (Reprinted from the British Journal of Psychology. Vol. VI, Part 2, October 1913), pp. 223-238. Cambridge, University Press.

Psychological aspects of the problem of atmospheric smoke pollution. By J. E. WALLACE WALLIN. (Mellon Institute of Industrial Research and School of Specific Industries; Smoke Investigation, Bulletin No. 3). Pittsburgh, University of Pittsburgh, 1913. 46 p.

Über Gewöhnung und Gewohnheit, Übung und Fertigkeit, und ihre Beziehungen zu Störungen der Stimme und Sprache. Von H. GUTZMANN. Fortschritte der Psychologie und ihrer Anwendungen, hrsg. v. Karl Marbe, II. Band, III. Heft, 1913, pp. 135-189.

Obliviscence and reminiscence. By PHILIP BOSWOOD BALLARD. Cambridge, University Press, 1913. 82 p.

Diary of a suicide. By WALLACE E. BAKER. N. Y., Albert & Charles Boni, 1913. 83 p.

Instituts Solvay; Institut de Sociologie, Bull. No. 29. Archives de Sociologiques, publiées par Émile Waxweiler. Dec. 26, 1913.

Il metodo degli equivalenti; contributo allo studio dei processi di confronto. AGOSTINO GEMELLI. Firenze, Libreria Editrice Fiorentina, 1914. 344 p.

Psychology and the medical school. By E. STANLEY ABBOT. (Reprinted from the American Journal of Insanity, Vol. LXX, No. 2, October, 1913). pp. 447-457.

Dynamic Psychology. By F. L. Wells. (Reprinted from the Psychological Bulletin, Nov. 1913, Vol. X, No. 11.) pp. 434-440.

Travaux de la clinique psychiatrique de l'Université Impériale de Moscou, sous la direction de Th. Rybakow. No. 2, 1914. Moscou, Printed by A. E. Mamontov, 1914. 645 p. (In Russian.)

La cyclophrénie (Psychose circulaire). Par Théodore Rybakow. University of Moscow, O. E. Rebakov, 1914. 182 p. (In Russian.)

COMMUNICATION

THE PSYCHOLOGICAL LABORATORY.
KINGS COLLEGE. LONDON, 14-1-14.

Dear Sir.—Mr. Winch has drawn my attention to an error which appears on p. 548 of my article On the Perceptive Types in the October number of your Journal. I should be much obliged if you would publish this correction.

In condensing the paper for publication, an alteration in the wording has made it appear as if Mr. Winch were not in favour of the strict mathematical treatment of psychological data. This is quite incorrect, as a matter of fact he is one of those who have always strenuously advocated the banishing, from experimental psychology, of its vague unqualified averages and percentages, etc.

The paragraph in question is worded as follows:

"The value of applying mathematical methods to experiments dealing with aesthetics is questioned by some workers in this field. However, if averages are employed, there should be some check on them before they are made the basis of any conclusions. Take *an example* from some fairly recent work. Winch bases his conclusions regarding the color preferences of school children, on averages which at first sight appear large but which on investigation fail to reach the required standard of significance. In other words they are less than three times their probable error."

The above italicised words are the source of the error. Mr. Winch's results were taken as *an example* of insufficiently qualified averages and not as *an example* of the work of those who object to mathematical methods.

Mr. Winch's results were chosen because they were the only averages known to me (in that branch of work) which were qualified by the insertion of the m.v. of each. The criticism intended was that the treatment of his data might have been carried further, and I very much regret that such a misconception should have been created.

Yours faithfully.

E. J. G. BRADFORD.

To the Editor, *The American Journal of Psychology.*

Georg Ernst Dürr was born in Würzburg in 1878. After receiving his early education in his native city he entered the University of Leipzig, where in 1901 he was appointed to an assistantship in psychology. In the following year he went to the University of Würzburg as Privat-Dozent in psychology; and in 1906 he was called to the professorship of philosophy at the University of Berne, a position which he held until his death. His writings include: *Ueber die Grenzen der Gewissheit,* 1903; *Grundzüge einer realistischen Weltanschauung,* 1906; *Die Lehre von der Aufmerksamkeit,* 1907; *Einführung in die Pädagogik,* 1908; *Grundzüge der Ethik,* 1909; *Erkenntnistheorie,* 1909. Professor Dürr is perhaps best known to American psychologists from his contribution to Ebbinghaus's *Grundzüge der Psychologie.* The first volume of this work had been published in 1902, but on the death of Ebbinghaus in 1909 it was found that only some hundred pages of the second volume had been written. Dürr took up the work at this point and completed the volume, which was published in 1913. In the meantime Dürr revised the first volume for the third edition which appeared in 1911; and he also revised Ebbinghaus's *Abriss der Psychologie,* fourth edition, 1912.

Edmund Burke Huey graduated from Lafayette College in 1895, and received the doctor's degree from Clark University in 1899 with a dissertation on the psychology and physiology of reading. He taught psychology and education in a normal school for a few years; studied in Berlin and Paris; and later organized a joint department of psychology and education at the University of Pittsburgh. In 1909 he returned to Paris to resume the study of neurology and psychiatry under Janet. On his return to America he became resident psychologist at the State School and Colony at Lincoln, Ill., but soon transferred to Baltimore where he was Lecturer on Mental Development at the Johns Hopkins University and Assistant in Psychiatry at the Phipps Clinic. Dr. Huey's writings contributed to the psychology and physiology of reading, and to the scientific study of mental deficiency. The partially completed manuscript of a third book was destroyed by fire during the early stages of his final illness.

Arthur Henry Pierce graduated from Amherst in 1888; held an instructorship in mathematics at Amherst for three years; received the master's degree at Harvard in 1892; studied at Berlin, Strassburg and Paris from 1894 to 1897; and received the doctorate at Harvard in 1899. He was appointed to an associate professorship in psychology at Smith College in 1901, and to the professorship in the following year. He was the author of: Experimental Research upon the Phenomena of Attention, 1892; Gustatory Audition, a hitherto undescribed Variety of Synaesthesia, 1907; Studies in Auditory and Visual Space Perception, 1901; an Appeal from the Prevailing Doctrine of a detached Subconsciousness, 1906. Dr. Pierce served as secretary of the American Psychological Association from 1908-1910, and as editor of the Psychological Bulletin since 1910.

Theodate L. Smith graduated from Smith College with the bachelor's degree in 1882 and the master's degree in 1884. She received the Ph.D. at Yale University in 1900, and served as research assistant to President G. Stanley Hall at Clark University from 1902-1909; she was lecturer and librarian in the Children's Institute at Clark University from 1909-1914. Dr. Smith collected and systematized a voluminous mass of data dealing with institutions for child welfare; her publications include the following: On the Education of Muscular Power and Control, 1894; On Muscular Memory, 1896; Reactions to Light and Darkness, 1903; Curiosity and Interest, 1903; The Psychology of Day Dreams, 1904; The Montessori System, 1912; Paramnesia in Daily Life, 1913.

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A SYNTHETIC GENETIC STUDY OF FEAR

By G. STANLEY HALL

CHAPTER II

III. *Psychic geotaxy*, or orientation to gravity, constitutes a distinct and fascinating theme of geneticism. Beginning with the tropistic level, it grows complex as we ascend the scale of life, and in man has many outcrops, conscious and unconscious, normal and morbid. Our protocol on this topic consists of questionnaire returns, letters, oral communications, notes from medical and other literature collected for sixteen years and embracing nearly three hundred individuals. Such data shed no light on the percentage of frequency of such phenomena, but although age is not always given, we can roughly estimate that some four-fifths of these cases are under thirty, and one-half under fifteen, that there are nearly three girls to one boy, and that save in a score and a half of clinical cases the reactions, although often very intense, are compensated and outgrown later. Again, while responses to the vertical direction are manifold, the factor of fear is predominant. Pronounced and persistent fear, too, is so obvious that while trained observers are desirable, the instructed lay reporter can furnish data of as real value as that accepted by students of heredity, clinical psychology, and anthropologists, although of course all such material needs to be sifted and weighed by the expert. The best method of presenting data like the above cannot be very exact, for tables and curves would involve too many arbitrary distinctions, but must consist of mosaic, of salient sample instances that outline a composite portrait of the chief traits involved and bring out type cases. Of course the adequacy of such a general picture depends confessedly much upon the psychological sagacity of the compiler. The following, then, is a characterization of my data, the best I can make in the space.

Young people often remember vividly the experience of being tossed, perhaps high to the ceiling and being caught, or of being trotted high on the foot or knee, or the baby jumper. Occasionally this seems to be the very first memory, and sometimes it is the only thing remembered of a dead grandparent or other early acquaintance. It is always exciting and nearly all children love it up to a point (varying greatly with the extent of the movement, who is responsible for it, a trusted friend or a stranger, and especially with the diathesis of the child), beyond which it becomes a fear or perhaps a terror, causing shrieks and even convulsions, making the child forever after hate or fly and hide from those who toss it. For some it has a unique charm and those who can do it are persistently importuned. Most children love to be high, although two were reported who before the end of the first year (it was said, improbable as it seems, with no experience of falling) could not even be lifted to a high chair, a table, or be carried. Infants, too, are instinctive climbers, often getting up stairs before they can walk; they cling to hair, beard, clothes, almost convulsively to save themselves from a fall. Immediately after birth, as Robinson showed, they can often support their entire weight by clinging to a cane, a power soon lost but which suggests arboreal life. The desire of climbing trees, fences and every accessible high object, and also that of jumping down on soft places like haymows is in some children remarkably intense and prolonged. Falls, on the other hand. may give an absurd dread of even small heights and set back a child's courage to walk weeks and even months. Trettien (Am. Jour. of Psychol., v. 12, pp. 1-57) in tracing the processes by which the child acquires the upright position and especially Mead (Ped. Sem., v. 20, pp. 460-84) have shown that the power to stand and walk involves a marked advance in the power to talk, in general intelligence, also in courage and independence. The first step may reveal any symptom of the height complex, charm, dread, respiratory, cardiac, muscle-tonus and even stomatic phenomena. Not only a fall but illness or sudden fright or consciousness of what it is doing may cause the child to react by creeping and hitching for weeks or months and thus lay the basis of height phobias later. Our data suggest that any sharp check of the infant's instinct to get up on end constitutes an obstacle to learning to be at home in great heights which may be compensated, but even then may leave the adult with greater liability to those peculiar panics that sometimes seize those who work at great altitudes. Teetering, swinging, spring-board leaping and diving, elevators, balloons, and even an aeroplane experience are found in our data. Some cannot bear to step on gratings, and two had a fixed and acute dread of cracks, one between paving stones and the other of those in clay where mud had dried, which latter suggested earthquakes or "the way in which proud Korah's troupe was swallowed up." Very common is the impulse, usually very sudden, to hurl oneself down from towers, windows, roofs, bridges, high galleries in church or theatre, precipices, etc., and not a few grew rigid, livid, clenched their hands and teeth, clung almost convulsively to railings or bystanders, or had to be held by their friends from plunging off in order to escape the tension by "ending it all" or "to see how it would feel" to fall or get the "beautiful sensation" of it, often in the hope of being upborne by their clothes, a parasol, flapping hands or arms like wings, or some absurdly planned but inadequate device, trusting luck to "strike in a soft spot," etc. The pure suicide motive in these cases is often a sudden eruption, it is such a good opportunity to die and have it all over in a moment. There is little doubt, however, that this jumping-off instinct in the young and old may lead to death without any real plan of suicide. There is sudden giddiness, nausea, tremor, gasping, or sense of smothering or a deep inspiration which may be a sudden revival of an habitual dream or nightmare experience of hovering and floating, and in certain cases a firm conviction that respiratory phenomena are the cause of the levitation. Some describe an alternating flicker of fascination and horror, and the impulse to jump comes, as it were, in the trough of the wave of charm. Some are cool at the time, as one was on the top of the Eiffel tower, but had an acute attack of shudders in recalling it later. Several described dread of going to heaven because it was so high and described how their horror of hell was that they were always falling. Several who had dreamed of flying often and had come to find it pleasant, grew to feel sure well on into the waking state that by a peculiar intake and holding of breath (such as fakirs use to hypnotize themselves for weeks, as if they were dead) they had learned the secret of ascension, and two planned dramatically how they would startle their groundling friends and an awestruck world by their discovery. The writer has had repeated similar experiences. Some who were never known to get up in their sleep feared they might do so and took precautions every night, one girl tying her wrist to the bedpost lest she should leap out of the window in a dream. Well terrors are common and some children for months and years can never approach wells or a certain one, perhaps believing it bottomless. This is connected with various well myths. Some for a time have a morbid dread of hills, and a few even of very slight acclivities and declivities. This, too, often appears in the myths connected with famous mountains. So stairs have a fear psychosis of their own, perhaps usually associated with painful infantile experiences, and which often cause absurd precautions or fantastic modes of going up and down. Skating and walking in slippery weather may become intensive fears embroidered with manifold fancied accidents. Many cannot ride horseback and a few will not trust themselves in a vehicle lest they fall out or the bottom of it fall out or there come a tip-over. Three of our subjects speculated about reversed gravity and had spells of fear that they might drop upon the world or fall off at night when it was upside down. Sometimes such fears are stronger for their friends than for themselves. Closely akin, too, are the manifold fears that things will fall on us, such as meteors, stars, comets, clouds, sticks of spent rockets, rocks if we are near a precipice, chandeliers, ceilings, books from church galleries, steeples, signs on the street or over store doors, belfries, towers, etc.

After making all possible allowance for exaggeration and every other source of error, such data lead the insightful student of childhood to the conclusion that these fears are far more common and intense while they last, even with normal children, than has been supposed, even though they be very fugitive. Again, they are often supposedly kept secret (although easily betrayed) from adults for fear of ridicule or censure. They seem most pronounced in girls. If yielded to with the abandon of the present moment, so characteristic of childhood, they often seem to die out of themselves, without very great effort at compensation. Still the devices for concealing them may be very subtle and ingenious, and the child with acute dread of wells, stairs, upper stories, and the rest, may devise an intricate system of defences and excuses, such as preoccupation, lameness, whim, convenience, etc., to avoid experiencing or betraying these dreads. Newborn infants which I have tested in a lying-in hospital, with no possible experience of falling, showed unquestionable physical signs of fear when lifted up and down on a cushion, and we may infer that all the subsequent hurts from falling only modify an innate orientation to gravity, which birds must be born with or they would fall over the edge of the nest.

In the act of falling through space the pressure of all organs on the tissues that support them is removed, and in being lifted upward all the normal pulls and pressures are increased. Changes of the pressure of the content of the stomach upon its base, which may cause nausea, also changes of the fluids of the semi-circular canals connected with vertical giddiness; of the brain and other organs or their supports, of the five or six foot column of blood upon the walls of the blood vessels, the absence of these in falling and their increase in rising, modifications of ocular and auditory sensations, cessation or augmentation of pressure against the soles of the feet, the podex or other dermal surface, in lying down:—all these are factors of essential although very varying value. Clinical cases teach us that with anaesthesia or with hyperaesthesia, and in some diseases of the eye and ear, may go marked sensations of levitation or heaviness, which are also modified even by changes in muscular vigor. The feeling of weight or lightness is thus a very changeable ground tone of many somatic impressions, too complex and too far submerged to admit of more than partial analysis as yet. The end of every drop beyond a certain height also is a hurt, and every individual acquires a plexus of these experiences, but the intense degree and the marked manifestation in some cases of the ambivalent charm-dread of up and down movements points unmistakably to a large inherited factor, and suggests that not only the experiences of the human stage of life but arboreal life among springy branches, when a fall meant death or serious mayhem, or possibly rising or falling in an aquatic medium have left their traces in some kind of nmenes or engrams. The pelagic stage of life of our forbears was certainly far longer than the dendritic stage, and the latter was probably far longer than that of life on terra firma. True, we can identify no organs that mediate these experiences and can only postulate some kind of psychic ids or determinants, but the inflation of swimming bladders in rising and the evacuation of air from them in sinking, and the gasping or suspensation phenomena of respiration in dreams or even fancies of hovering, the soporific effect on infants of gentle vertical movements, sometimes even with the cradle lullaby of "Rockabye, baby, among the tree tops," the climbing, clinging instincts of children suggestive of monkey life, suggest that, after exhausting proximate and individual causes before having recourse to remoter racial ones, the former fail to explain the normal and far more so the abnormal phenomena in this field, which current psychology has so ignored, but which geneticism can never do in any field. The facts seem to suggest that hovering (anakataesthesia) is to be distinguished from falling sensations (or eluaesthesias), which are later, the former being vestiges of aquatic, the latter of arboreal life. Of course the older of these experiences is to some extent reproduced in the foetal state with nearly equal fluid pressure on all sides, to which embryonal stage psychoanalysts (Ferenczi and others) have of late had recourse. Our ancestors floated and swam long before they had limbs, and they breathed by gill slits, which the foetus reproduces long before the development of lungs. If we have here phenomena which suggest different modes of movement before limbs, and of breathing before lungs, why should this seem more surprising when it occurs in exceptional cases, than the persistence of rudimentary organs or their hypertrophy in monstrous births? To doubt it is to conceive the soul as more limited in its backward range than the body, if not, indeed, to think too meanly of the former. Although we cannot demonstrate rudimentary processes in the same tangible way that we can rudimentary organs, may we not in fact have before us here some of the very oldest elements of our psychic life, reminiscent echoes of the primeval sea, on the one hand, and vestiges of dendritic life, on the other, a pristine outcrop of the factor of the space sense itself? If not by recourse to phylogeny how shall we explain either the hovering or the falling phenomena, the innateness of which seems now beyond all doubt?

In learning to stand and walk the child rapidly repeats a long stretch of racial history. Up to an average of about twelve months it creeps in many ways, forward, backward, sideways, also hitches, rolls and crawls, and even after it has just learned to walk may, if in a hurry, revert to creeping, or if in a great hurry, to the still more primitive and still faster method of rolling. The first standing and stepping are almost always unconscious, with the desire to get something, as we throw better if we fix our attention, not upon the movements but upon the target. Consciousness of what is being done, attention of others called to the act, a little surprise, a fit of laughter or of anger, looking out and up and realizing how high they are and what a view they command, and thus getting a little dizzy, may often cause children at this stage to suddenly fall. Their automaton can walk; their mind cannot. A bad tumble, an illness, a long journey when it must be chiefly carried, mental fatigue or discouragement, may set it back for weeks or even months, so that it must learn to walk over again. After a sickness the child at this stage often manifests great surprise that it can no longer stand or walk. A few babies in our record can first stand or walk in one place only, or perhaps only for some single purpose, but diversion is almost always necessary for the first step. Some at this stage can walk only if they clutch their own dress, as they did that of their mother when they took their first step. One could walk only if it put on its father's cuffs, which it reached up to in its first successful effort to stand. Successful standing often seems to give a certain familiarity with heights and to prompt to still greater ones, so that the climbing instinct which precedes walking may be reënforced. The first successful efforts to stand and walk bring much elation. The very excess of joy at it may cause the toddler to go down. Many records show marked improvement of temper and still more of health. The sum total of exercise is greatly increased, and so fascinating is the new balancing performance that the child often indulges it to the point of fatigue. The range of interests widens. The arms are now suddenly freed from the work of locomotion and are flung about in joy or used to maintain the equilibrium, and there is almost always a strong instinct to put them to use by carrying things. The habitual line of vision is now horizontal and higher. There is a marked access of courage, venturesomeness, and interest in things distant. Indeed the child who has learned to stand and walk enters upon one of the chief prerogatives of man's estate. It is an accomplishment which involves many complex coördinations and many changes in the incidence of strains and pressures in all parts of the body. The head is now balanced on a vertical column and not carried by the muscles of the back. The shoulders, which have been pushed back by normal creeping, are now supported, as is the weight of the arms, by the bony structure, the weight of the viscera presses downward, the hips and knees, hitherto almost always flexed, are now straightened, and many other far-reaching physiological changes occur.

In a large anthology of cases of loss of the power to stand and walk in adults, which have been collected mainly since about 1888 (for the disease seems to have been unknown in antiquity), we have many very striking analogies with the stages of learning to stand and walk, though after a comprehensive inspection of the literature I can find no recognition of this inverse correspondence. Astasia-abasia might almost be described as unlearning to stand and to walk. Nearly all its symptoms in children are found (in a smaller and more obscure way, as must be the case in their undeveloped lives), in attaining the upright position and the power of locomotion; or conversely expressed, the symptoms of the disease in adults are more pronounced, more prolonged, and more isolated than in the process of ascent in children, and the order of their occurrence is often inverted. The patient dreads a wide outlook, so that this disease is very often intimately associated with agoraphobia. Even standing, and still more, walking, may cause flushing, palpitation, giddiness, timidity and tremors, very like the first success of the child, and in both we have a similar propensity to stagger and to clutch at anything for support. We have the same ability to both stand and walk unconsciously and unnoticed, and the same collapse upon sudden realization of what is being done. In true astasia-abasiaphobia there is the same intactness of muscles and reflexes, strength, spinal centers, and perhaps the same power to make all the walking movements when lying down.

The typical syndrome of this phobia, according to Delarme, whose characterization of it is the best to date, is a systematized functional paralysis of standing and walking engendered by an emotivated idea of the impossibility of these functions, existing in various degrees and sometimes, though not necessarily, accompanied by lesions of the locomotor apparatus. The lower centers may be normal. The fact that such patients can walk when their attention is distracted from the act shows that the thalamic and probably the Roland centers are not affected, but that the trouble lies in the higher, psychic regions, some think in the center of Grasset. Knapp describes a form of fear which he calls intention psychosis, and one without fear which he calls an association neurosis. There may be astasia-abasia. with no fear, a distinction that Sainton stressed, but at least alarm at this condition, which is of course from dread of the act, is almost inevitable. The obsessive idea that the patient cannot walk soon arises. Its onset, if not due to an accident, is rarely sudden, but muscular atrophy slowly supervenes and cure is unusual and very slow. It is not aboulia, as Magnan thought, and is rarely *Schrecklähmung* or of purely nervous origin, as Binschwanger opined, even though a fall may occasion the outbreak. Recent studies have laid more and more emphasis upon the psychic elements of this disease. Sometimes the memory images of how to walk seem to be lost. In some cases these are intact but the patient stands and walks only if in a narrow street or if there are no people in sight or none look at him, or he sees no one he knows, or he may go down only if spoken to. One patient had read of a case in which the bones grew so brittle that they broke under the strain of standing before a mirror, and so developed a phobia of a similar catastrophe for himself. In one oft-cited but suspicious case, a hypochondriac who thought his limbs were made of glass, fell, said " Smash," and died. Even in its slightest degree there is always a loss of buoyancy and indeed of all its symbolic meanings that tiptoe upward and in their place a feeling of more or less heaviness or depression. The posture is almost always stooping or crouching, with hands and arms tense, the feet spread, the movements clumsy, involving mainly the fundamental and but little the accessory movements and muscles. The act of standing and walking is performed only with exhausting effort and is innervated in an abnormal degree from the higher centers, with much overflow of energy expressed in increasing tensions. Very rarely are there distinct ataxic or spastic movements of the hands or feet, and these probably never occur unless lower centers are involved. This loss of the power to stand and walk may be purely hysterical or the result of suggestion, and the symptoms are of all degrees and may be grafted on almost any lesion of the locomotor apparatus, though the element of fear and dread in some form is very rarely absent. As the disease advances we usually have a limitation of intellectual interests and even of the use of language, reversing the augmentation of these, which, Mead showed, followed in the child the power to walk.[1]

Is the acute anxiety of these patients too great to be explained by the loss or impairment of this very fundamental power, and, if so, may we here assume an atavistic reënforcement from arboreal experiences? In tree-tops the upright position with the arms flared about like balancing poles brought dangers of a disastrous tumble. The risk of walking upright along limbs without holding on with the hands was of course in part, at least, compensated by the prehensile power of the foot and perhaps the tail, both of which were lost when life on the ground began. The use of these powers would render the animal secure when stationary, but in rapid movement, as from one tree to another (which Wallace observed in ourangs might sometimes reach the rate of four miles an hour), safety involved much and pretty constant handwork, with considerable foresight as to which branch to choose for a path to the next tree, and often with considerable risk of falling. The locomotor efforts of patients with abasiaphobia, with their stooping and straddling, waving of arms, clutching of hands, constant incipient falling and recovery, the anxious fore-looking for the next few steps, the selection of a goal and the staggering toward it, and especially the impulse to grasp everything supportive in their way, are highly suggestive of tree-life. The same is true of the agoraphobia symptoms. The tree-climber just getting down to life on earth has only too much reason for new fears. Above it was secure from most of its enemies, save tree-climbing serpents, which all apes dread (a fear which often persists with morbid intensity in man). On earth safety by flight or speedy recourse to a tree was the only salvation from danger which might arise in any direction. So the agoraphobiacs skulk near buildings, with flitting fancies of taking refuge to steps, climbing to porches, open windows, up pillars, or it may be cliffs, flag-poles, on high walls or fences, or performing other yet more impossible feats if emergencies arise. Hand and arm movements seem often to go with these wild fancies. In the first terrestrial experience the arms, if unused in locomotion, are generally useless except for carrying the young and in manipulating food. Their very size and weight, greater in anthropoids than in man, changes the center of gravity and endangers equilibrium, unless they are waved to steady tottering steps, a function for which their very length would make them more effective than the human arm. I cannot find in all the copious literature that a true case of astasia-abasia has been subjected to psychoanalysis, and until we have a collection of cases thus studied the final demonstration of the above thesis must wait. As to the symbolism of hysterical dread of falling, explained as really dread of falling into sex-error, even if we grant that such cases occur, we must remember that the psychosis of falling long preceded, so that we really have here only a late graft, made in the autistic level, and the symbolism is hardly more than a figure of speech, such as uprightness, downrightness, altitude, plunging, and many more. Man's interest in the vertical dimension of space, in zenith and nadir, in the heavens and in the depths of the earth, would perhaps never have been possible, had he been a quadruped, however philosophical.

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[1] Strohmayer, Über das Symptom d. Astasie-Abasie. Monats. f. Psychol. u. Neurol., Bd. 12, pp. 315-28, distinguishes four types of abasia-astasia: (1) unconscious or hysterical, coming often as a surprise; (2) hypochondriacal or resting upon the conscious delusion of inability, occasionally with paraesthesias and hyperfatigue, disturbances of vision, palpitation; (3) affective or due to anxiety that has no content and that is indefinite, like agoraphobia; and (4) imperative where when a patient tries to stand or walk he is seized by the idea that he cannot or must not although quite conscious of the absurdity of the idea, which is quite in contrast with the case in (2) above.

IV. *Fears of losing horizontal orientation.* The instinct to know "where we are at" is very fundamental. One of its simplest, and from questionnaire returns, one of its most common manifestations, is found at all ages, but especially in childhood in suddenly awaking from deep sleep with a sense that we are lost. Our data abound in cases where children awake and cry out because they cannot tell where they are, whether in bed or not. Some are speechless or motionless if they awake crossways or diagonally in bed, and think they have been carried elsewhere. Sometimes when they have rolled out on the floor they creep around to find the bed or anything else and perhaps are dizzy and sick with fright. Some awake with a sensation of being in the wrong bed, that the windows are misplaced or that the furniture has been moved, and the feeling of being turned around gives them a panic. Some are nauseated if they cannot instantly locate familiar objects in the room when they wake. Sometimes they are aroused from a dream by a sense of being surrounded by thick walls and with a sense of suffocation. This is more common when a child sleeps in a strange place. This experience may be very acute and oft-repeated for years and then gradually die out. It seems to be rarely connected with antecedent dreams but more with some slight convulsive experience, and this suggests *pavor nocturnus* or night terror, of which very likely it is a highly specific form. In most of our cases, however, there seems to be no physical shock but a sudden feeling of being lost in an utterly strange place, and it is mitigated by sleeping with others. Whether, like *pavor nocturnus,* it is more common during dentition, or with a neurotic heredity, or with a predisposition to epilepsy or hysteria, or an irritated condition of digestive organs, cannot be determined from our data. The child very rarely screams or snatches wildly at things, or makes frantic defensive movements, and there is rarely any sign of catalepsy or either eclampsia or eclipsis, but the child usually soon re-orientates itself and falls asleep with little recollection of the experience in the morning. Childish dreams of being lost are not infrequent. They seem to grope in strange places among strange faces, and seek in vain for anything familiar. These phenomena are far more common in children, because of their very limited acquaintance with persons and places and their greater dependence upon them, and perhaps their more intense gregarious nature because of its limited range. Young animals prematurely separated from the nest or home prepared for them by their parents experience this same shock of strangeness and its results are dangerous and often fatal. In a few cases this experience was connected with the child's suspicion that its parents wanted to get rid of it or that it was supposititious.

Some nervous children can, with great difficulty, learn to sleep in a strange place, and must have their own bed in its own wonted place. Some are long haunted with the fear of being lost if they are at all off their own familiar beat. Perhaps they dread they shall lose their way to school, can with difficulty learn to go cross-lots unless they can see the goal, hesitate at every turn lest they may take the wrong road, are dizzy where they must turn a curve or a corner, and painfully try to keep tab on all the angles or perhaps dread they shall get in the wrong classroom. The smallest forest and perhaps even the word 'woods" may arouse peculiar dread. Some children cling almost frantically to their mother when they turn any angle. Some cannot go off with their mates because of the incessant fear they should hide or be lost for an instant. A few who had seen little of outdoors until three or four found the wide, wide world oppressing and overwhelming, could only venture a few feet from the door, and could attend only to distant things, which to children very rarely can be made to excite interest, with pain. A few show an interesting ambivalence between this dread and the passion for running away. Some children, soon after they learn to walk, start off dropping perhaps first headgear, then coat, footgear, and wandering on and on as if all psychic anchorage to home had been lost. The sense of being pursued makes them go both faster and farther. Then in some cases after a painful and prolonged experience of this kind the opposite feeling of timidity at being any distance from home, supervenes. It would seem in our returns that ambidexterity often involves difficulty in telling right from left, and thus intensifies the obsession of corners, at which some children pause and hesitate lest they get turned around. More are obsessed by dread of losing the points of the compass. A college professor was unhappy for a year in Berlin because east always seemed west, and the sun went the wrong way. A few awake in some anguish until they know which way is north, and one had a chronic dread that this loss might come suddenly and stunningly. Some dread it most acutely in a forest. others in the country, and yet others in the city. It may suggest the fear of dying alone or of starvation. Some must have in hand, or at least in mind, a map of every new place, or they get confused. A very intelligent farmer had never been but twenty-two miles from home because he disliked strange places and people, thought he might get lost, and was certain he never could sleep save in his own bed. Some rather curiously "realize eternity" and think on the end of time, space and the world. In one case this brought weakness and palpitation, serious thoughtfulness, and a ritual of Bible verses and hymns was developed to lay this strange obsession. One experienced creepy feelings whenever the thought of vastness came upon him, until at twenty a reaction came and he thought he believed in annihilation, brooded over the idea of being universally diffused at death, and thus overcame his fear. There can be but little doubt that children's dreams of place are very vivid, so that in passing either way between sleep and waking the environment of one melts either suddenly or gradually into that of the other. "Where am I?" is often one of the very first questions of morning consciousness and sometimes we see strange oscillations and mosaics of these states in hypnagogic phenomena. Everything in the room may be a lighthouse or buoy to guide them into safe harbor from their far dream voyages and so cannot be moved without confusion. Much depends upon the suddenness of transition into the waking state. Children differ immensely in the speediness and certainty with which they learn their environment, and probably even more so in the vigor and courage with which they explore it. Some timid home-clingers almost suggest an atavistic relapse toward the early forms of sessile life or attachment to the parental body, and remind us how slow and late in the animal series well-developed locomotor organs arose. Over against this stands the running away, migratory instinct, so common in fishes, birds and nomadic races, also the spring fever which is so intense in northern peoples after their long winter, when they feel they must be up, out and away, a trait which Holmgren has so well described, which has cropped out in the great human migrations, and the *scholares vagantes,* to say nothing of tramps, explorers, globe-trotters, etc. The primitive eye, ear and nose, at least, are in part organs of orientation to direct movement. The earliest infant memories show that the natural configurations of even slightly rolling ground, and especially of salient landmarks are among the earliest and most persistent of all forms of memory. Closely connected with this are all the phenomena of nostalgia or homesickness, which is so very complex and has never yet been analyzed, but to the strange anchoring power of which memories of persons, buildings, landscapes, food and feeding, contribute factors and to which striking geographical features certainly contribute, and which seem less in those wonted to desert or prairie. As to the function of ambexterity, distinguishing p from q, may cause dizziness, although p and b are easily distinguished. We do not know whether those least ambixterous most tend to walk in a circle.

Certain it is that the danger of getting lost in primitive tribal life must long have been very great, and it must have prompted to a very careful study of all landmarks, not only on the earth but in celestial regions. Early man was surrounded by *terra incognita,* which his fancy peopled with all sorts of horrors, and we doubtless have in the above experiences phylogenetic elements from this period. Animals, the axis of the body of which is horizontal, very early orientate between head and tail directions, as cephalization and forward movement increase together and with them the sense of from or toward whatever is localized. This orientation must have been a very primitive factor in the development of space feeling. When the erect position was assumed, however, it became far easier to turn about on a vertical axis and to feel turned about, and thus horizontal orientation became harder. Finally, cosmic dizziness, or fear of disorientation in regard to the direction in which the solar system is moving, seems to go with a sudden and intense vastation of the ideas of the size of the universe and is doubtless connected with the infinity psychosis. This is not uncommon in the teens, when some have distinct agoraphobia at the thought of spatial infinity, while the claustrophobiac type of mind is relieved to find open spaces between the stars, or hopes that space is a void instead of being full of ether. Some connect this with a Nirvana impulse or the Indic cult of cooling passion by sitting crosslegged and saying "Om," or the intellectual appetency for pure and undetermined being. (See this subject more fully treated in my "Adolescence," v. 2, pages 159 *et seq.*)

V. *Fears of closeness.* These are somewhat more specific and here our data are yet more copious. Many children remember with great horror some experience of being shut up in a dark closet or a chest, or a room alone. A group of Western boys made a cave, with turnings, nearly sixty feet long and with many sinuous and labyrinthine turns, and initiated other boys by leading them into it and then deserting them till the terror of even the bravest was so great that adults had to intervene. To some the least sense of confinement is stifling. Some cannot endure closed carriages or small rooms, must have windows and doors open, can never enter a strange room if the key is on the outside, etc. Sometimes when shut in a room the sudden feeling that the door is fastened springs up. A few will never allow a carriage door to be latched but keep it slightly open on some pretext. Children who have had acute experiences of being shut up may have desperate dread of all narrow places lest they crush. Some dread fog and shudder lest it may grow a little heavier, clearer, denser, and finally choke them, or are haunted by fear of gas and go around every night to see if every jet is off. A child at three first noticed mist and was terrified, thinking God had spread a veil over the earth that would never lift. Other children feel nausea, asphyxia, think the sky is falling, in a fog; some have similar fears of suffocation at warm weather and perhaps their every thought of death is strangulation. Long is the record of fears of being buried alive, with accounts of horrid distortions of bodies dug up that have come to life. To some anything snake-like suggests coils about the neck and choking. Some awake before daylight and fear the sun will never rise. Occasionally we have a record of a child who cannot even sit in a corner. Others have phobias of all caves, ravines, gorges, dense forests so that they cannot draw a full breath. Others woke up with the bedclothes over them and this has occasioned claustrophobiac symptoms. Some dream of being screwed down in coffins and awake in a cold sweat. A boy in the teens wrote out minute details for his burial in case of death, and not a few want tools or food or drink buried with them, the coffin padded at the bottom, large and roomy with holes for air, with a tube so they can speak and breathe, a bell fastened to the hand, etc. In a few cases even a very strong odor brings a hot, close, smothery feeling. One cannot wear a ring unless it is very loose, and if it sticks the least bit in coming off she is in a panic. One can never even have a medal hung around her neck. One cannot even hear or read such words as suffocation, strangling, hanging, oppression, confinement, tyranny, constraint, and is occasionally overcome by them suddenly when night is falling. One after a slight trance, in which he heard all that was said but could not move, was for years obsessed by the dread of being buried alive, but this was compensated by the thought that God, who had cured that spell, would not let him die in another. One found her eyes slightly stuck together on awaking, and had a panic that long persisted. Boys often have a horror of getting their heads in chancery in a fight, getting sat on or crushed in a football scrimmage, and to some the dread is like that of losing their liberty, as in the case of a colored boy who, when choked in a fight, cried out with Patrick Henry, "Give me liberty or give me death." Thus we doubtless have in these experiences the basis of a very elaborate symbolism vitally connected with man's passion for unrestricted liberty.

Hunger for breath, which begins with the first filling of the lungs after birth, demands considerable room for air, and this increases with age and restriction of it causes incipient asphyxia. Even slight dyspnoea predisposes to claustrophobiac states of consciousness, although any association of apnoea and the over-saturation of the blood by oxygen does not, so far as we know, dispose to agoraphobiac symptoms. In all these deep-lying analogies of sensation, which seem better illustrated in sleep than in waking although with laws common to both, the state of the lungs, blood or heart, seems to suggest the concept, more often in dreams, while the reverse appears to be more characteristic of the waking state. Breathing is a specialized form of skin respiration and arose from it. It takes longer to strangle young than adult animals, and this would probably be also true of man. Some children habitually sleep in closed rooms and even with the head under clothing on account of fears of darkness, and this may predispose to claustrophobia later or at least to the sthenic smother effects and even globus to which shut-in experiences predispose. Very faint and remote suggestions are often sufficient to evoke its symptoms, a valley, fog, warm weather, a snake, odor, a word, the thought of tyranny; and all this shows the immense range and power of the symbolism here involved and how our highest ethical aspirations are those that strike their roots deepest into the most primitive experiences. The very interesting and sudden impulse of prisoners long confined to break out, run amuck, smash things or their own heads and fists, the vagaries of certain ventilation cranks, the psychic torture of being compelled to remain too long in closed spaces, reversions to tents instead of walls, mountain fever and its inspiration, root here. Sully Prudhomme wished the world was not round but stretched out infinitely and was continuous with stars and sky, so that we might travel to them if we took time enough. He found the limitations of living on a sphere oppressive. One might construct a more or less accurate measure of these sensations somewhat as follows: the subject might stand safely based on a high steep pyramid or even a tower or pillar, and an umbrella-like horizon might be opened with its apex just below his feet, till midway it made a flat horizon all about him. Then it might gradually rise, closing upward, till without moving he was in a pit. Thus we might measure in degrees where claustrophobiac and agoraphobiac symptoms began. We can hardly conceive aquatic creatures to have these experiences save at least in a low degree. Air-breathers really require many times the cubic contents of their bodies in air for respiration hourly, and this, like other of the above elements, may have been a factor in the early development of the spatial qualé, perhaps *a priori* to the sense of sight or even of touch. Certain it is that orientation in a space is exceedingly complex, and it is probable that when analysis can penetrate a little farther we shall leave Berkeley, Kant and modern epistemology far behind and make room for genetic factors of space perception not hitherto adequately considered but which will open up the problem of the *Ding an sich* of space perception to a larger if less formal treatment.

My suggestion in 1896 of a phylogenetic root of both agora- and claustro-phobia, which has been adopted by Mercier and others, was as follows:—that it was once very useful for our arboreal ancestors to be near tall objects, predominantly trees (to which modern tree-dwellers have again had recourse), and which has perhaps left other vestiges in the form of tree-worship, folk-lore, myth, and the very unique feelings which children have for trees (see Dendro-psychoses, by J. O. Quantz, Am. J. Psy., 1898, v. 9, p. 449), and that the latter phobia harks back only to a much later period (and is hence more common) of the troglodytes. This is at least in line with the present strong genetic trend of modern psychiatric thought. When from the high, breezy, prospective trees our forbears took refuge in dark, smoky, and probably unwholesome caves with perhaps only a single exit, so that escape from danger had to be not by flight but by fighting, the contrast was extreme. Gelineau thinks that agoraphobia is more common in high altitudes than in low, and gives a case where it was evoked even by the picture of a mountain when the patient was in bed. Sonnier has shown that these phobias may both exist in the same person. Alexander (Zts. f. Phys. u. Psy. d. Sinnesorg., 1911, Abt. 2, v. 45, pp. 153-96) found that of 118 new-born infants, most of them less than 24 hours old, 92 responded to rotation much like adults by head movements after and especially during rotation and by nystagmus afterward, which he interpreted as a vestibular reflex. Children love to play with their static sense of equilibrium by somersaults, rolling down hill, swinging, whirling till they fall, etc., to see the world spin around. Pirouetting dancers and whirling dervishes who turn for hours to symbolize the revolutions of the heavens show how controllable vertigo for rotation is. Perhaps whirling insects, aquatic forms, dancing mice, birds, also enjoy these sensations. In children they may be recapitulatory or prophylactic because they develop compensatory processes that safeguard them against this polymorphic symptom later. Certain it is that after middle life vertigo becomes not only a very common but a much dreaded symptom. It may occur not only with troubles of the ear (as Kreidel, Brück, and K. Beck have shown) and accompanies many disturbances of the stomach and pneumo-gastric nerve (the heart and circulatory system, the bulb and cerebellum), but it is also common with epilepsy and *petit mal,* neurasthenia, traumatic neuroses, tabes, kidney troubles and adrenalism, nasal and laryngeal disorders and as Dor has shown is common in glaucoma and it may be of toxic, cortical or suggestive origin (Wilson, N. Y. Med. J., Nov. 25, 1911, *et seq.*). Oppenheim (Neurol. Centralbl., 1911, p. 290 *et seq.*) thinks that true chronic vertigo does not rest on any material change in any organ, but its only constant basis is an irritable condition of the nervous system or a neuro—or psychopathic constitution. Here belong many anomalies, such as periods of remission and exacerbation, its persistence after lying down, the fact that walking is rarely impossible although the ground may seem to give or rock, that riding may suppress instead of enhance it, that it may persist for years with no compensation whatever. Friedlander thinks chronic giddiness may be psychogenetic and a result of suggestion, and that its chronicity may arise gradually. On account of its great complexity, its connection with so many parts, functions and diseases, differential diagnosis, although greatly desired and needed, has not yet progressed very far. Laboratory studies are mostly confined to ocular and vestibular giddiness. Introspection could probably go further and distinguish these, not only from each other, but from dizziness of gastric and perhaps circulatory origin. But there is always the question whether back of all these specificities there is not a very generalized form, from which all its many varieties have sprung. Even in normal, every-day life, questionnaire data indicate that in its slighter and more transient forms it is far more common than had been supposed, as if man were not yet very securely established in the upright position. Many in their ordinary avocations suffer from occasional onsets of lightheadedness and develop often through months and years rather elaborate defence and compensation systems and methods, such as fixating stationary objects, stooping or holding the head down, slowing down the gait of walking or giving it a slightly spastic character, making occasion to take hold of things or persons, clenching the hands, gritting the teeth, swallowing, deep breathing, making some excuse to bend, sit, or even lie down, using smelling salts, etc. Ewald shows that giddiness may cause slight errors of accommodation or convergence that modify our impressions of distance and cause us to make mistakes in grasping things. There may be anaesthesia of the soles of the feet, that causes the patient to wobble if he stands with eyes closed and causes actions that have been compared to what would probably result in a change of the center of gravity, or gazing into a mirror may reduce the static sense. In such cases the fear of betraying the symptom to others seems most common. Next comes anxiety lest one should fall down or against something and be injured. And third and least frequent is solicitude concerning the meaning of the symptom itself, for the subject's health, a sequence which, for the interests of hygiene, ought to be reversed. Not only do such sufferers avoid high or dangerous places, but the confidence and straightforwardness expressed by their gait suffers impairment and they take on a more timorous attitude.

The tendency to approximate the body-axis to horizontality (to bend, stoop, sit, lie) in general mitigate, while converse upward movements enhance, this symptom. The agoraphobiac, too, cowers and tends to sink, the claustrophobiac to be up and away. Golla (Proc. Roy. Soc. Med., vol. 5, Neurol. Sect., p. 123) showed by use of a tippable rotating chair that with closed eyes his subjects were often quite unconscious that they were being rotated, but when the motion ceased they experienced a very marked after impression of movement in the opposite direction, although Stein has shown that with slow partial rotation angular distance can be rather accurately estimated, even in a box shutting out air currents. Mulder (British J. of Psychol., 1911, v. 4, p. 204) found a rate of rotation and reversal in which the initial sense of dizziness could be suppressed before it reached consciousness and a complete fusion result. Abel showed that after checking passive movements of the whole body in a straight horizontal direction there was with closed eyes a slight illusion of an opposite movement, while Mach, with a large Atwood machine to lift and lower the human body, showed at least a slight sense of movement in the opposite direction afterward with closed eyes, an experience much more marked in descending into and rising from mines. Cruchet and Moulinier, who interrogated aviators, concluded that the great changes of blood pressure they experience with hypertension, fear, vertigo, and afterward somnolence, do not depend so much on height as on the rapidity of transportation in space. The rate of speeding straight away, the pleasure of so doing which is neutralized by fear and its symptoms, varies much with health, age, disposition, and most of all, with experience, for it is very educable. We have neurotics who fear to walk fast or to ride much faster than they walk. Some seem to fear not so much an accident as the effect of the exhilaration of rapid transition upon themselves. Passive riding is essentially a human achievement, and in man it reverts to, revives and intensifies the infantile experiences of being carried, and the psychoanalysis of the future may refer the individual's love and dread of rapid transportation back to early experiences of being carried, wheeled or otherwise moved from place to place before or without the effort of walking. Every step, from the first animal or boat that primitive man made use of down to the most modern methods of transportation, has brought great and new exhilaration, and also probably has excited new dreads in neurotics, whose fears today express an early stage before this series of discoveries began, where every translocation meant personal effort.

Vertigo is reduction of orientation in space. It occurs to some degree wherever there is incoördination between movement, whether reflex or voluntary, and the normal result of change of sensation it causes. Thus a weight which I find to be far heavier or lighter than I expected, and so gave too little or too much energy to the effort of lifting it, a slight cramp or tic that moves the limb, head or eye unconsciously, an unwonted afflux of blood to or efflux from the head, any very unusual distribution of blood or of nervous energy among the parts and organs involved in the somatic sensations that underlie the very ego itself, any disturbance of which gives a sense of unreality of self or of the world and thus lays the foundation for epistemological giddiness, any ailment that affects the normal dynamics of the body, especially in its relations to the outer world, any unwonted incoördination between the sum total or the relations between them of efferency or afferency, may give vertigenous impressions. (See second edition of Hitzig's "*Der Schwindel,*" 1911, pp. 141). This may be not only very slight and momentary but unconscious. Any disturbance of the normal balance between the subjective and objective, the dynamic and receptive processes, a sudden access of either adequacy or inadequacy of response to the environment, the flashing sense that what we thought real is unreal or vice versa,—all these cause psychic and may bring physiological giddiness. This sensation thus has a very wide symbolic and metaphoric field of meaning. It is the genetic basis of man's impulsion to find his own place in the universe, to know himself and his world, to evolve a system of theory as well as of conduct and behavior that fits him to fill his place in nature.

VI. *Rabdo-ballisto-aichuro-acro-merintho-phobias.* Many think the *stick* was the first tool or instrument and that man became man when he could wield it. The question whether the higher apes can strike or use a club has its advocates and its opponents. However this may be, the stick principle was a discovery of inestimable pregnancy, probably unsurpassed by any other invention down to the present time. It was the first great achievement of the hand when its long evolution was accomplished. The power to strike with fists and hands made man formidable to his fellows and to lower creatures, but to swing a stick through a large area and therefore with greater velocity, to transform the point of impact or concussion from the sensitive and softer hand to the hard, heavy end of a stick, even before it was edged or pointed, gave to man's hand a weapon which made wood and metal as well as enemies, brute or human, his servants. Moreover, as Lotze said of the cane, it extended the limits of his personality, for we feel ourselves at the farther end of everything we grasp, especially when it hits an object. Thus many primitive tools and weapons came to be mounted on handles, mostly of soft material, and so now lost, but which greatly increased their efficiency. Thus in the first stages of its use man became preëminently the striker, the hard hitter, and later the prying lever, the whip, the scourge evolved. From this first tool-weapon also came the mace, the caduceus, the scepter, symbol of kingly power among men and gods, and even the divining rod. No wonder, then, that the rod became the center of a complicated cult in rabdomancy so that magic power has been ascribed to it. Again, an infant under a year is prone to strike with almost anything in its hand that can be used for that purpose, but towards and after puberty the passion becomes so strong that Acher thinks that when it culminates boys are almost powerless to resist the impulse to strike animals, mates, weeds, flowers, posts, pictures, to pound objects. The child's conception of the chief function of the parent, the teacher, the policeman, is often to inflict blows with ferule, billy or birch, and Gulick lists many games such as baseball, tennis, hockey, cricket, etc., in which the chief charm consists in hitting or striking.

All this pleasure at the near end of the stick has its counterpart in the pain and fear at the other end. Even dogs fear the man with a cane, horses the man with the whip, and the civilian is more careful not to provoke the soldier or knight with a sword by his side or to exasperate the tramp with a cudgel or shillalah. For ages the stick in its many forms has been the agent of judicial punishment and private castigation. In view of the above facts we should expect to find morbid fears of the rod and this is not uncommon, especially among children. Even a single blow from parent or teacher has power to turn love into lifelong aversion and hate, and we have records of scores of children, more or less delicate, who have been thus made sleepless, nervous, or have run away. In a recent article on children's suicides, out of nearly 250 cases it was computed that nearly 20 per cent. were due to fear of anticipated or mortifications after inflicted punishments. The history of flogging and scourging in criminal procedure, prisons, armies, navies, as well as in schools, is long and its literature is copious; so it is in the story of the wager of battle, of personal encounters, from the stick fights in many parts of the East to the refined codes of the modern swordsmen duelists. Thus the impulse to hit and the dread of the shock of being struck have had a long and varied history. There can be little doubt that rabdophobia or morbid fear of the rod or even mastico-phobia strikes its roots in some way into man's ancestral experience, as does the complementary tuptomania or the passion for hitting. Its countervalent or opposite is seen in the passion of the flagellants and the self-scourging of penitents, which almost suggest rabdophilia, under the spur of the instinct of justice to give each sin its due meed of pain. Even boy champions court blows in conflicts because the pain they inflict is far less than the pleasure that comes from the excitement of combat. Thus in the rod complex we have a good illustration of several of the fundamental principles of the dynamics of man's instinctive emotional nature:—(a) its primordial beginning, coeval with that of man itself; (b) its intense and varied pragmatic value; (c) its progressive evolution and differentiation; (d) the very strong feeling that the rod has a value that far transcends man's comprehension and which thus expresses itself in thaumaturgy, conjuring and the rodmancies; (e) its strong ontogenetic recapitulation in the modern boy; (f) the morbid forms of it in cases where the instinct to use and the fear of it are abnormally developed; (g) the countervalent overcoming of the fear in heroism, penance, self-castigation.

When after learning to hold a hard object, e. g., a stone, in the hand, and to strike with it, the stone slipped out of grasp and went on in the direction and with the velocity given it by the hand, *throwing* was suggested and soon learned. This power no brute acquires. It was the first palpable form of action at a distance and marked a new and pregnant epoch when man became the *thrower,* hurler, propeller. From this sprang later all the ways of projecting things and of ballistics. Angular rapidity remaining constant, the longer the arm or the arc through which the hand moved, the greater the velocity of the missile, and hence came the sling and the throwing stick, in all their varieties. The throwing passion in boys is often so strong, especially in snow time, that windows, vehicles, street lamps, passersby, domestic animals, trees, posts, and any conspicuous object arouse an invincible temptation to throw at them. Thus the street urchin is ever re-celebrating the acquisition of this new power. Out of this soil also grew skill in many games, ball, quoits, billiards, tenpins, archery, tennis, polo, battledore, etc. Boys grade their throwing power by distance and accuracy and some one has enumerated the scores of things they throw in the order of preference. Here, too, myth records the most incredible prodigies of strength, speed, skill. Things animate and inanimate are thrown across the sea, to the moon and the sun as if gravity and the law of resistance and decreasing momentum were superseded. So entrancing was the throwing idea that the wildest fancy has sometimes been let loose to supplement fact. An Indic hero threw a downy feather through an enemy's breast and his rival threw a leaf into a rock so deep that only a tiny scar could be seen where it entered, and a high and solitary mountain shows where a hero picked up an island and tossed or heaved it onto his foe. Thus it seems as if fancy grew delirious and ecstatic when man learned to throw. Dogs and other animals have learned to fear and fly when boys or men even stoop to pick up a missile, and balistophobia or morbid fears of missiles is not at all uncommon among timid children, while in adults it splits up into special fears of injury of person or even property by a multitude of swiftly moving objects, from lightning and bullets to snowballs and stones, against the scathe of not a few of which criminal law in every land has provided.

The *point* has played a great rôle in the evolution of man, but its psychology has never yet been written. When primitive man first learned to bring his whole energy to bear in thrusting a sharpened object at and into his foe, he became almost invincible. The sharper the point and the greater the force that pushed or impelled it, the greater, of course, the penetrating power. The teeth, beaks, claws, horns, nails, of animals taught this principle, and it is no wonder that arrowheads are among the oldest of the arms of man. The point must, of course, be strong and of hard material in order to bear the strain, and so every step of the progress from sharpened sticks and stones to ever harder metals, was epochful. Victory over beasts and human enemies was in proportion to the perfection of pointed weapons, for they must often pierce tough, shaggy hides or shields of foemen.

When man had not only learned to put a handle upon his point but to throw his sharp weapon, and still more to do so with all the momentum acquired by the invention of the bow, he became still more formidable in both hunting and warfare. Ancient myth abounds in accounts of darts, spears, swords, arrows and javelins of wondrous acuity and impelled at incredible distances with a penetrating power that nothing could resist. Apollo brought pestilence with his darts and Cupid love. Jove's thunderbolts were hurled from Olympus with strange power to pierce and destroy buildings, trees, rocks, animals, men. No armor could resist King Arthur's Excalibur or the Volsungs' good blade Gram or the spear of Ithuried, and the very rays of the sun were apperceived as darts, as were the shafts of death. Here, as always, with the danger grew the terror. The psychology of war describes but does not explain the strange panic that seizes even hardened warriors, that arises before serried lines of victorious troops advancing to a charge, and no one who has not felt the terror of a broadsword or dagger can realize the mad fear that it inspires. The dread of anything liable to penetrate the body has thus had a long period of incubation, while on the other hand the instinct to pierce it, which points suggest, often crops out even in normal children, with their mania for pricking not only others but themselves, as Acher (Amer. Jour. Psy., v. 21, p. 128) shows. Still more striking vestiges of these complementary psychoses appear in the imperative impulses of the stabbers and in aichuro- or belono-phobias. The one brings raptures sometimes suggestive of the frenzy of modern warfare, throwing up blood pressure and bringing general erethism, while the other involves a no less frenzied horror at the very sight of any sharp point or edge that could be used to pierce. Even a penetrating look, a stare, the power of which several contemporary psychologists have actually thought it worth while to investigate (See Coover, Amer. Jour. of Psy., Oct., 1913), a finger pointed menacingly or thrustingly at us, especially if approximated boringly toward infants, in whom it often excites the almost spasmodic manifestation of the tickle sense (the excitability of which shows in both tickler and tickled the psychoneural scars left in the phylum by its accumulated experience with thrusts),—all these belong to the same genetic category of piercing the dermal surface which bounds our physical ego, outside of which lies all that is objective and inside of which all that is subjective. Traces, then, of these old shudders, which should have long been left behind, still survive and occasionally erupt in natures in which they are uncompensated in unreasonable horrors of forks, thorns, toothpicks, nails, even pointed fingernails, ornaments and sometimes pictures of anything that might penetrate the body.

That suggestions of piercing and being pierced may come to have a predominantly sex meaning in some cases the symbol analyses seem to have made certain. The sexual soul is a great synechdochiser, but the fact that very many children long before puberty have fears of points, and the fact that the chief pain done to man and his history was by non-sexual piercings, shows the limitation of this interpretation. With the invention of gunpowder this psychosis was reënforced and also given some new features. The effort involved in piercing an enemy's body is now practically eliminated for chemical takes the place of muscular energy, and both the range and power of the projectile and the difficulty of defense are enormously enhanced. So we now have a passion for shooting something and the often morbid dread of firearms, greatly intensified as it is by the noise of the detonation. The modern warrior and hunter stabs afar off with a bullet, the velocity of which vicariates for the sharpness of the piercing point, and hunted animals soon learn with considerable accuracy the range of every new weapon. But this new zest and fear have behind them the psychogenetic momentum of the paleolithic age when the discovery of the mighty power of the point contributed so much to develop man's intelligence and to place him at the head of the animal kingdom.

To this end the *edge,* twinborn with the point, also contributed much. Wood, stone, hard and harder metal with ever acuter finish, on to the razor and microtome from the crude wooden and claystone ax to obsidian, bronze, and to the steam chisel that cuts cold steel like wax, swords tempered and acclimated to cut armor as well as hairs and gossamer, and every kind of cutting tool, blades of magic power to cleave trees, rocks and mow down men, the kit of the modern surgeon that enables him to dissect the living body, and if we add to this all types of the saw, shave, adz, ax,—all these are simply prominent chapter headings in the long history of culture. Here, too, myth has run riot in its tales of blades so sharp that they could cut gossamer in the air. In one Arabic tale a warrior waved his scimitar before a guest many times at which the guest wondered and laughed, but on being told to shake himself fell into slices, the weapon being so swift and keen he did not know it had touched him. All the instincts connected with the episodes of this story have spontaneous outcrops today in the boy's love of the knife, of whittling, carving, in girls' interest in scissors, which Acher has brought out in such strong light. The edge mania too has its phobia and its countervalent *Umschlag*, for whatever gives power also brings danger and so besides aichuro- and belono- we have acro-phobia, which latter is rarer, as we should expect, because from the first cutting is less dangerous than piercing, but which is of interest because both loved and feared. Thus we have here another epoch-making discovery which still reverberates in the secret recesses of human nature in a baffling way.

The *string* principle has played a great and almost wholly benign rôle in the world's history and has an anthropological literature of its own. The first grass, withe, bark, used to tie things led on to knots, weaving, the bowcord, games and puzzles galore, and even to a language as in the case of the quipu. String exists in nature and is used by spiders, by weaver and sewing birds; crows fear it and are kept from fields of corn by strings around it, as if they had learned its possibilities as a snare. Man has put it to very diverse uses. Of this interest we have a rank and spontaneous outcrop in children. Acher says that they have no spontaneous interest so strong from five to eight as that in strings and cords, which he thinks is now neglected but should be systematically utilized in inducting the child in the many mechanical, industrial and even intellectual possibilities in it. Games with string are hoary with age, and they have been used in many religious rites and ceremonies, chants and songs. Bolton found 97 forms of cat's cradle and thinks that they go back before the pyramids and are almost universal among savages as well as civilized people. All textile arts and everything knit evolved from the first thread, and weaving, embroidery, wicker, crude or refined needle-work, basketry,—these have always been in some form one of woman's chief accomplishments. Sewing is now often prescribed as a sedative or placebo for tired nerves, which, like so many other cures, is due to reënforcement from older evolutionary strata. Ropes, wires, chains, cables and all species of the genus string and its many utilities make the question unusually interesting whether it too is an object of morbid terror. Merinthophobia, however, although rather rare, is directed largely to the uses of the string principle in binding and restricting liberty. Before jails and prisons, not only criminals but slaves were tied. Captives were tied up, mutilated, tortured, or for sacrifice perhaps burned or hung. Without the string principle the very word bondmen would have had little meaning in its literal, and perhaps less in its symbolic sense, and our language would have been otherwise much the poorer in its tropes and figures of speech. Perhaps even the modern idea of freedom would thus have been less developed but for this principle. Victims of this rare obsession, of which children quite often show traces for a time, feel a sense of suffocation at the sight of a string or as if it were a serpent or might form a noose or tighten about their neck, chest or limbs. They hear, think or dream with horror perhaps of animals tied in their stalls and burned to death in a conflagration or of being drawn under water by some entangling mesh or cord. They reverize how small, strong, sinuous or tight a thread, twine or wire might cut into the flesh or enmesh some part, and with these thoughts there is almost always labored breathing. On the other hand as there is a mania for or inclination toward about every object for which there is a phobia, so here we find in some, especially adolescent girls, a passion for tying themselves up, often so ingeniously that it seems as if another must have done it, until they are truly helpless. In such cases it seems as though there were an instinct to be passive and to put themselves into the condition of being freedomless victims. This of course does not involve any feeling toward the twine that ties but is only based on the craving for a situation in which they are unable to save or defend themselves against imaginary quasi-inviting victimization.

These five, the stick, missile, point, edge, string, and their derivatives and uses constitute a closely related Pentateuch of psycho-genetic categories of somewhat peculiar character and interest. (1) First we have a very obvious and quintuple repetition in the modern child of the very early culture history of the race. So strong are these impulsions to re-live long ancestral stages that modern pedagogy should utilize far more than it has yet learned to do this momentum, so much of which now goes to waste, by training this rank interest earlier in children, first in the way of the broadest psycho-motor development and later in a more specific industrial way. (2) While most normal children show inclinations to use about all five of these objects painfully upon others, and a natural reluctance to have them used thus upon themselves, they strongly tend to fall into two groups, the inflictors and the sufferers, the one tending towards mania or philia, the other towards phobia, both of which in exceptional cases become pronouncedly morbid. (3) Childhood is by far the largest and richest field for studying all these phenomena, most children having for a time the passive, active, or both attitudes to a degree that would be distinctly pathological in adults but which in them is generally outgrown or successfully compensated later. Adult phobias, and especially manias, though more persistent and isolated, are far more rare. We can never understand psychoses or even neuroses without understanding children, who seem to carry in their nature the keys of both, so that for a time they can understand these aberrations better than adults although as they mature to normality this portion of their lives becomes a sealed book. (4) The sex interpretation of the above difference as sadistic or masochistic represents a far more special case of bifurcation of human nature although it is an exquisite illustration of a far more general law, and although it is true that in this wider field girls more often tend to the phobiac and boys to the philic or aggressive diathesis. (5) Each may show *Umschlag* phenomena or conversion into the opposite. Those most fearful of them may feel and yield to the impulse to strike, pierce, cut, tie, and those with a philia for doing this to others may sometimes suddenly, but more often gradually, become so tender and sympathetic that they take fantastic precautions against injuring others in these ways. Thus the one comes to love what he hated and the other hates what he loved, for every impulse has its unconscious countervalent opposite which, if the conscious impulse goes too far or dominates too long, may itself come to the fore while the former regnancy sinks below the threshold, where it may be able to express itself only in dreams or automatisms while it is biding its time for another revolution that will bring it into power. (6) If phobias are thus sur- and revivals of traces in us of the salient points of long ancestral experience, we cannot understand the one without the other and may sometimes reason both ways. We may infer from strong and in the individual inadequately motivated impulses something as to what experience the race must have passed through. Conversely, when the latter is known, we may confidently expect if not predict outcrops of similar phenomena in childhood and in neurotics. Perhaps ultimately we may thus foresee and anticipate ethnology on the one and paidology and psychiatry on the other, and postulate for pragmatic reasons what science will later demonstrate as genetically true.

VII. *Ophidiophobia.* The most universal of all animal cults is that of serpents. There is hardly a land or primitive race upon which it has not left its traces. It pervades every mythology and enters more or less prominently into every known system of religion. It has left its marks, monuments, mounds and architecture. Of all animal cults many think it the very oldest. It enters into most ancient rites and is central in many of them, from the Eleusinian mysteries to the Moqui snake dance. The symbolism of which it is the center ascribes to serpents two opposite characters, one good, the other bad. From ancient Chaldea as a center it irradiated widely in the antique world, and new evidences of its prevalence are constantly found. The serpent tribe has had many fanes and temples, and the Adams County (Ohio) serpent mound is about a thousand feet long. There is a great and growing literature upon ophidian folk-lore, superstition, worship, etymologies, and totems. Serpents guard treasures, are connected with various divinations, give knowledge of underground treasures and passages and of the future, teach agriculture and healing herbs, and have played a great rôle in medicine. Serpents, large or small, with or without wings and legs, are thought to have infested various lands. Some are both wise and cunning. They change their form, protect maidens, infest certain wells, dens, mountains, fens, jungles, caves, islands, perhaps live at the bottom of some sea or lake, and are often associated with fire, lightning, charms, death, and there are many tales of ophidian lovers and of maidens who have given birth to serpents. Again, there are mythic monsters galore with more or less serpentine traits, dragons, hydras, gargoyles, pythons, behemoth, sea-serpents. In some tales great serpentine creatures have created the world and they are often closely allied with death. Dresslar collected 33 typical contemporary superstitions concerning them, and they may infect dreams and appear in forms of mental alienation as well as delirium tremens. If we include all creatures with serpentine traits, like griffins, lizards and other creatures, we can understand the suggestion of Gomme that some of these popular fancies may be connected with remains of the great triassic saurians. The conquest of formidable creatures of this general type is often among the great achievements of culture heroes, from Hercules and Apollo to Beowulf and Saint George.

For genetic psychology it is a challenging problem to account for this proclivity to reptilian, lacertian or more specifically serpentine psychoses. Are present-day experiences with serpents together with traditions and their social inheritance sufficient to account for contemporary man's attitude toward creatures of this type, or must we assume some kind of psychophysic heredity, going back perhaps to the time when to our tree-dwelling forbears serpents were the most dangerous of all their enemies.

Before we can answer this question we must glance at a few obvious facts. First, what are the chief traits of ophidian life that are most impressive? They are first the uncanny form of movement, without legs, by sinuous windings and writhings. Snakes seem to slip and glide along in a way that seems very mysterious, as if by some supernatural or magnetic power, and they pass over water in the same way. Moreover they slide along with their entire belly on the ground, under the grass or leaves beneath our feet, noiselessly, perhaps, half covered, and usually protectively colored, so that they escape notice until they are upon us. The superstition is widely prevalent that they can climb up our bodies as they do trees and enter them, especially those of females. Again, their eyes are bright, lidless, beady, and generally apparently motionless, and are thought to charm and fascinate not only their prey but man. Slowly as they often move, some have the power to spring, strike, grasp, and coil with great rapidity, and folk-lore abounds in tales of their clinging about legs or throats and choking, and we have many a record of neurotics who feel symptoms of strangulation at sight of them. Again, their bite is apparently very trivial but their venom may kill suddenly and in a way that seems magical. Thus the cobra slays its thousands annually. The reptile's coldness of blood, its long periods of torpor between meals, hibernation, the great age often ascribed to it, its unique power to shed its skin, which has made it often a symbol of resurrecton and immortality, constitute an ensemble of qualities that is very unique. Thus if we knew all about the history of totemism, those writers may be correct who assume that in every land we should find traces of serpent totemism as widely diffused as is the snake race itself, so that it may be assumed to have had a certain preference of selection for this function.

My collection of children's morbid fears of snakes and things that creep and crawl like them (using only those cases where we have the parent's, teacher's or child's assurance that there had been no known or special experience with them) now numbers 94. Some cannot step off the path onto the grass or on leaves without very cautiously and perhaps constantly looking for snakes. Some search the bed every night or fear or fancy that they may twine over chairs or tables, imagine all kinds of hissings and rattlings, which they ascribe to unseen snakes, fear to drink in springs, lest they swallow their eggs, perhaps dream of them persistently, have a horror of toys that suggest them, will not touch or look at a book in which they are pictured, faint at the sudden suggestion of their presence, or even the sound of the word snake, and sometimes the horror of caterpillars or worms is connected with these phobias. A botanist even yet thinks imperatively of snakes when he is out collecting plants. Others dread them most when they go in swimming, start at every stick or string that suggests them, have a sudden panic of them when climbing rocks, fancy they might meet one when up in a tree, imagine them in wells, ditches, sewers, water-pipes, outhouses, are seized with a sudden shudder that they are inside their clothes, perhaps down their backs or breasts:—types of fear which are much more common with girls, whose dread of them is more spasmodic, prompting flight, while boys usually conquer their fears by a passion to kill which seems unusually strong in man at every age, who seems ever bent on fulfilling his mission of crushing the serpent's head. Children often invest them with marvelous powers, e. g., they can stand erect on their tail or with but very slight support, can roll like a hoop with tail in mouth, can run up and perhaps penetrate the body, leap great distances, can strike or sting with the tail, emit horrid odors, paralyze or hypnotize with eyes or movements of the head and tail, think they lure birds, suck milk, come to life after being killed, that the tail dies only at sunset, and in general show facile credulity to all sorts of *Aberglaube* to which they are exposed and to which their feelings cling, perhaps imperatively, long after the age of reason has dawned and they know better. Like so many other forms of fear these are very common and often in morbid intensity in normal children for a season but are usually completely outgrown, leaving no trace in adult life, unless strain or other experiences initiate reversionary states which often bring a strange recrudescence of them and show that their vestiges persist far below consciousness. Perhaps despite the denials many or possibly all of my 94 cases really had some experience with snake-like creatures, although the majority of them seem to have been bred in the city where serpents are rarely seen, and a few were under two years old and therefore could hardly have heard unfavorable mention of them. It seems to me we should infer from all this that the childish psyche is peculiarly susceptible to suggestions of this type of creature made up of its unique traits, and perhaps analysis would show that all focussed on one or another of these qualities.

If there be an ophidian *Anlage* or a tendency to a characteristic psychic attitude toward the snake family that is in any sense phyletic, this need not be a complete ophidian pattern linked with a group of motor preformations or a definite type of feeling ready to be set off by the first experience. But all we have to assume is a greater tendency to develop any group or even a single one of all the elements of the snake complex. Would the psychophysic organism of the individual develop in the same way and with the same facility in this direction as it would if there had been no ancestral experience? Heredity may give only a certain preferential facility of association between two or more of the traits that make up the serpent character. A snake-like form and the sinuous form of movement, e. g., or beady eyes, coldness—any of these may tend to fuse with each other more readily because they once did so in the phylum. Again affectivity may be more easily aroused by such impressions from the receptors and its resonance a little clearer, or the motor centers of certain cries and spasmodic movements a little more impressionable from these sources. Again when any single quality is focussed on, as brilliant eyes or sinuous movement, it alone cannot be assumed to be an adequate excitant because the impressions of certain other traits that make up the ensemble must be assumed to co-work, at least unconsciously.

Students of the intensity of Mendelian heredity of physical traits make this influence diminish very rapidly as we go backward, parents, e. g., giving 1/3, grandparents 2/27 of all. Thus if we were to go back 700 years with 20 generations, when the theoretical number of ancestors was 1,048,576, the contributions of all the ancestors of the generation farthest back would be but very little, so that if we were to go back some thousands of generations, as Haeckel estimates their number, to our tree-dwelling forbears, this influence would be infinitesimal. Hence on this theory the probability of transmission of a trait to modern man from his Simian predecessors would be almost nil. But neither the theories of Weismann nor the figures of Pearson fit this case, and that for the following reasons: First, children do in fact often show the persistence of traits of body in the scores of rudimentary organs according to Wiedersheim. Again, the arboreal stage lasted very long and this is a potent factor in persistence. But third, and chiefly, the central nervous system differs from all others in that it is par excellence the organ of registration or of physiological memory. It is there that the traces of ancestral experience are stored so that almost nothing that was ever essential in the development of the phylum is ever entirely lost. Hence suggestive as are many physical traits of our racial history, the intangible psychophysic traits must be assumed to be both far more numerous and more indelible. While these faint tendencies often crop out in a behavioristic way, by far the most of them need some stimulus of individual experience to awaken them, and still more exist only in a slight facilitization of impulses or permeability of nervous centers, lability of molecules or neural tensions, or as preferential reënforcements in one rather than in another direction or manner:—in this case from the organization and activation of the innate psychic momenta or other activities that constitute the highly complex reactions to fear. To make a violent not to say absurd supposition that everything spherical a generation or two ago became self-moving and just as dangerous as serpentine creatures have been during that time, can we assume that man's instinctive reactions to this would be as pronounced or as fearsome as those to serpents even if tradition had grown up about the former as rank and as fearsome as those about serpents? Is there no preformation or predetermination or are such proclivities to *Stellungsnahme* themselves genetic and innate, unorganized though they are at first? All admit that we inherit the capacity to shudder and to fear, and none claim that we are born with ever so generic a snake image any more than we are born with *Anlagen* to other general species of animal life, but this is not all the story. There must be something between, a missing link, if only in the form of the reduced resistance between the ensemble of the several snake qualities as they impress the sense centers and the inborn type of fear response. That is, these two are functionally nearer together than the imagery of dangerous self-moving spheres could be brought in a few generations, and this constitutes in some way or degree a determining tendency which we might call a theomorphic fear matrix which can be impregnated only with this specific type of imagery. There must be something here which we do not yet know and of which some such term may be a provisional figurative designation of a quaesitum, and here in the present stage of genetic psychology we must leave it, awaiting the light of further knowledge.

Finally, as in amatory fetichism, affection tends to focus on one trait of the loved object—hair, smile, complexion, etc.—so here (as is the case in other phobias) one serpentine trait is often focussed on—eyes, mode of progression, coiling, biting—to the relative exclusion of other traits. Both these fetichistic phenomena are far more common with the young. and the fears which seem to show the most of them are those that though perhaps morbidly intense are, in the process of growing up, entirely eliminated. Why this stage of fragmentation of this fear, this hyper-accentuation of one trait among many? Does not this apparent lack of cohesion of essential traits indicate a disintegrative tendency among the factors that enter into the ophidiophobiac matrix postulated above? We answer no. We see in this phenomenon only a case of the wide ranged law of over-determination by which one factor in the complex is pushed into consciousness and made more prominent there by the other parts of it which remain below the threshold. Coiling, e. g., would not be so unduly prominent in the consciousness of the child who has this fear as it often is, did it not draw on the psychic energy of the other submerged components. Indeed the trait that is focal in consciousness may and perhaps must be the least effective in arousing the motor response and the feeling. The psyche often hangs out a signal to show what has happened, and this signal is often at first an epiphenomenon which later comes to be itself cardinal. First came organic response and then the awareness focussed on the trait that was least effective, which thereby became thereafter not only the most patent but the most efficient stimulator. This suggests too that the image in its totality is not the chief excitant of the serpentine fear attitude, and that by a kind of afferent metonomy a part of it may, and very commonly does, do duty for the whole, but also that the energy of excitation is thereby increased.

VIII. Turning now to samples of a very different group of phobias, the literature on *ailurophobia* or morbid dread of cats usually describes it in adults. Rare in its extreme, it is so common in its mild form that a large proportion of mankind take sides and either like or dislike cats at the same time or by turns. The presence of cats in every known land and age shows that the ailurophiles, among whom women predominate, vastly outnumber the ailurophobes. Its relation to man is based chiefly on its attitude to small rodents, especially rats and mice, which have always been commensals and parasites of man. Both love and fear of cats are far more common and so best studied among children and young women, whose feeling toward them is based on other than their utilitarian qualities. In my collection of ailurophobias of varying intensity among the young, many causes, both vague and specific, appear, from "because they are loathsome and disgusting," to "because one once scratched and bit me badly." Most children have suffered some more, some less, from their teeth and claws, but their even chronic and strong aversion is usually overcome and it is only a very myopic psychology which accounts for all aversions in this way. We have a number of specific causes, more or less distinct. (1) Some affirm that their repulsion toward them is "because they walk so stealthily and noiselessly that you do not know that they are about until there they are." "They step around so slyly that you have to watch them to know where they are." "They may be out of sight, hiding under or behind anything." Hammerton observed a cat leap to a sideboard, explore it and thence to a completely set dinner table, worming its way between delicate wine-glasses, touching nothing and making not the least noise, when a dog would have upset things. This, then, is the first indictment against them and is the reason why the rats in their fabled parliament resolved to bell them. Our criticism of their stealthiness is a rudimentary form of the same feeling we should have more intensely if housecats were the size of their big congeners, lions, tigers, leopards, panthers, ocelots, etc., which once commonly and still in some lands made man their quarry, somewhat as cats do rats. When the Kaiser visits his relatives at Buckingham Palace it is said every room in his suite must be thoroughly searched by a trusted official as to whether cats lurk unknown in some nook or corner, for this the war-lord cannot abide. The cat does not patter, bark or bay like a dog to announce its coming but steals unawares on its unsuspecting victim until presto! the "sense of presence" is sprung upon him, but too late. This factor seems predominant in some ailurophobes.

(2) Another closely related, uncanny, gruesome, deeply graven element in our cat complex is our appreciation of their power to make such great and sudden springs or leaps. In our returns children fear them "because they can jump so far"; "they pop up on the outside of any window and scare you"; "they hop in your lap or on the table or anywhere without any warning"; "they are so quick you do not know what they will do next"; "when you play with them they may get mad or scared and go for you"; "our tom-cat is quicker than lightning, and I saw him jump two rods through the air so quick you could not see him to catch a robin." Thus their saltatory power is often magnified to mythic dimensions. The cat to children is a salient illustration of the shock-inflicting power of being jumped out at and this is another ingredient. Practically every vertebrate that in fact or fancy may suddenly spring upon and overpower us is one of the Felidae.

(3) Its nocturnal habits intensify the dread of both its creeping and leaping traits, and this is intensified by the omatophobiac tendencies. "Its eyes are so shiny and glistening at night and in a dark cellar that you can see nothing but two glary balls of fire," etc. Living things upon which

the wonderful optic organ of the cat tribe is unswervingly focussed (for it is thus they have acquired a trait of attention and persistence in which Romanes thinks they excel all beasts) have usually been in grave danger. Folk-lore says that they thus charm, fascinate, hypnotize their prey. Only a fraction of the creatures who have experienced the baleful influence of its evil eye have survived to tell the tale. Hunters caught and mangled by the great cats have described the horror of their eyes as fiery, bloodshot, piercing, and some have thought they owed their lives to steadfastly staring the beast out of countenance in a kind of preliminary battle of eye to eye just before the beast sprang. Naturalists always marvel at the power of the cat's eye to see at night as well as by day, and even imitations of it are often used as charms.

(4) Next comes the homuxophobiac element or the fear of claws, together with odontophobia or fear of teeth, which is very akin to it. "A cat can run up you and stick its claws in your eye and tear it out if it wants to." "A lynx could grab you by the throat and rip your clothes off and tear your insides out with its hind paws." Several infants, which had loved cats, on seeing them yawn, show their teeth and stretch out their claws, were shocked into more or less persistent dread of them. "A cat can chew up bone and bite right through your finger and never let go." "A cat can get on your bed when you are asleep and bite in and suck all your blood and breath out." Thus the cat makes a strong appeal to the acuophobiac potentialities of juvenile humanity. Writers like J. Wesley Mills and Cornish mention the great delicacy and deftness with which the cat uses its forepaws, and the very early stage in its development in which this power is acquired, and call it almost manual. Save its ancestor the hyena, the cat jaw is relatively the most powerful known.

(5) The cat's vocal powers are no less exceptional and uniquely varied. It can growl threatingly, purr (a "whispered growl" as a girl termed it), it can miau plaintively and wheedlingly in asking favors, and this if it has to be too long repeated may rise to an angry and despairing yowl like the angry cry of the baby. It has a remarkable register of shrieks in its combat with its fellows although often silent when battling with dogs. It has also a crooning note inviting its young to its dugs. Very unique is its hiss and spitting, which is highly developed in some of its wild congeners and which some think it copied in order to add an ophidian terror to those which its own powers could create. These seven types of phonic expression for as many different sentiments

are all in the highest degree expressive, not only to its own species but to man. There is much reason to think that all infants have to learn not to fear even the most specific of these utterances, while some soon come to be very effective danger signals. Altogether this vocabulary shows the cat's nature to be emotionally rich and diversified although it is almost proverbially changeable and unreliable. Animal vocalization expresses feeling, not intellect, and the child lives so largely in the world of the former that its *rapport* with the feline soul is made much closer by this quality, so that it anthropomorphizes the cat and learns far more of the animal soul from it than from the more humanized and less composite psyche of the dog, for the cat has rightly been called the most emotional as well as the most vocally accomplished of mammals.

Besides these more readily rubricized elements of children's ailurophobias, our records show many other special features. One lady feared " nothing so much as the silent to and fro movement at the end of a cat's tail," which she fancied might even bite her and always watched for and dreaded it. This, too Cornish would say had a viperous suggestion. Another had a " horror lest a cat's hair get into her food " for if she swallowed it she would die. Another was doraphobiac but for the fur of the cat only, " because it prickled and emitted lightning in the dark," another because it was " dirty and was full of everything filthy." Two children feared they would be turned into cats, one long fancied it was a cat, went on all fours, miaued, ate, drank, and acted generally like one. Many cat dreams were reported of big, bristling, spitting, crouching, round-eyed cat-like monsters, and one of a brindled cat prowling near as if it still wore the protective stripes of the tiger in its jungle as tabbies do. Dreams of spotted leopard-like or older ailuroid forms do not occur in my collection. Whether the dreams of animals show any tendency to correlate in the forms of their object with those with which the human phylum associated we do not at present know, despite the reversionary tendency often shown in dreams. The power of cats to swim well despite their inborn hydrophobia and to do so without learning, their ability to climb trees, their love of heat which makes them cuddle up to man and sometimes to learn to sleep on the backs of horses and on the ribs of dogs, which man often interprets as a sign of affection when the cat is only seeking a warm place (Ellis says that man to the cat is a tree, his legs are stumps for scratching against, his body a trunk to climb, his arms branches that sometimes drop savory food), their moderate educability and power to learn to do mechanical feats, which Thorndike and Perry have studied, their strange homing instinct, which still rests more upon tradition than controlled observation, their vitality suggested by the phrase that they have nine lives, their care of their young and power to transport them from place to place, the marvelous rapidity with which their young develop:—all these together make the cat an ensemble of unique qualities uniquely combined, so that the individuality of the species stands out in a very marked way, and so that we see that the child reared without knowledge of cats lacks an important foundation for understanding human nature, and so that it is hard to see why in no language we have a single paidologically devised cat-book. While the gregarious dog adapts itself to man, even to his moods and whims, the cat has been but very little changed by him and remains always itself and it is we who have to adapt to it. It is still a half wild animal, living with us. It still makes its toilet alone, unaided by mates, as all gregarious animals are, and it does so still however clean it may be washed, as it did in its original parasite-infested forest home. Besides hissing, Robinson has shown at length the very viperine coloration that in the marking of Manx and striped varieties of cats when they are curled up asleep makes them a close imitation of serpents, with a special mark on the thigh to represent the serpent's head. With its ears drawn back and its mouth upon its own head he also shows how strikingly snake-like it is.[1]

A Berlin teacher of physics in a *Realschule* suddenly found that one bright fourteen-year old boy shuddered, was pale and nauseated and seemed unable to force himself to touch a new cat-skin, used to generate frictional electricity. The boy at first could only say that he feared there were vermin in the pelt but this was evidently a pretext. He was reasoned with and coaxed, and finally was able to force himself to gently touch the cat-skin with the tip of his fingers, and at length seemed to pretty well overcome his phobia.[2]

Weir Mitchell sent out a questionnaire on ailurophobia, to which he obtained 169 replies, one-third worthless. Some of the others he dismissed as worthless cases were of cat asthma, of which the symptoms were not felt for perhaps twenty minutes after being with a cat.

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[1] Yerkes, R. M., and Bloomfield, D.: Do kittens instinctively kill mice? Psych. Bull., v. 7. 1910, pp. 253-263. Perez, B.: Mes deux chats: fragments de psychologie comparée. 2d ed. Paris, Alcan, 1901. Cesaresco, E. M.: The place of animals in human thoughts. New York. Scribners, 1909. 376 pp. Passim. Perrens, M. F. F.: Memoires des mes chattes. Rev. Scient. v. 12, 417-424; 461-467; 491-494. 1899.

[2] Ein Fall von Mysophobie, von Dr. Krassmöller. Zts. f. Kinderforsch ung. Nov., 1913. Vol. 19, no. 2, p. 104.

In such cases there may be actual liking for cats, and it seems rarely to be what the victim of the trouble calls fear, although we should distinctly rank all such cases as organic fear based on ancient experiences, even though it is the odor that may have an obstructive and crampy effect on the nasal passages. In 31 cases Mitchell was convinced that the cat's presence could be felt even when it could neither be seen nor heard, and found the signs of fear were sometimes acute. In some instances the victims claimed they could smell the cat. In others this was the case in childhood but the acuteness of smell had diminished with age. Others were confident they did not smell and could not tell how they knew of its presence. Mitchell opines that the olfactory disturbance, dim though it was, was sufficient to arouse respiratory trouble without reaching the cortex or coming to consciousness as an odor. Why cats should cause this fear he makes no attempt to explain but cites the case of a well-known tiger hunter who would tremble and flee before a kitten. Hughes (Alienist and Neurologist, Feb., 1907, p. 64) thinks cat phobias should be limited to those who have never had any painful experience with cats but would not include fears of wild cats. He thinks there is both a superstitious and hereditary dread but he believes it is always a symptom of deteriorative change, for it is often more acute when health is impaired or after great brain stress and may vanish upon recovery.

Defending himself from the charge of being a "ghost-smeller," the late Elliott Coues (Science, Aug. 6, 1886) intimated that the entomologist Scudder's phobia for cats, which made him aware of their presence when he neither heard, saw, touched or smelled them, was telepathy and that the asthmatic symptoms they caused in him, which resembled purring, might be explained by some biogenic odic, akasac, nervauric emanation by impinging upon the delicate Scudderian sensorium, which focussed his mind and gave rise to a psychic storm of feline origin, a transference belonging to the same genus as that which in other sensitives was established by the sense of presence of a ghost. Professor Scudder in a letter to me explained that his feeling for cats was not fear but that the odor of them brought on asthmatic attacks, that he was not affected by a cat on the other side of a closed window but the effluvia of other animals or even woolen blankets that had been in contact with them caused his symptoms, that he could ride on but not behind a horse for the same reason, that cat odors even if almost unconscious barred him from many houses. Professor Burt Wilder sent me many cases of morbid fears of cats by eminent contemporaries and had collected many notes and intended to write a book upon the subject, he himself being extravagantly fond of cats. A New York physician was so unnerved by the sight of a cat in his visits, especially if it was beside a patient, that it interfered with his practice. He then forced himself to stay in a room with a cat, the presence of which he could detect when it was unseen, and at last was able to touch and thus abated his aversion. A Madison Avenue girl had hysterics if she entered a room where there was a cat even if it was unseen. Her friends hid pussy on one occasion in a room where she called, and when she entered and declared it was present, vowed there was no cat about. She insisted, and when its presence was finally admitted, fell with a shriek into a dead faint. Sometimes it seems to be the smell and sometimes the horror is the stealthy, silent way in which cats move. I have a detailed story of a large Maltese cat that visited a room in a New York tenement where it gave birth to kittens. Meeting a dog in a hall, it attacked and vanquished it, became furious and sprung upon the five-year-old child of its hostess, biting, scratching and inflicting forty bleeding wounds, some of them serious, before the mother, with the aid of neighbors, was able to beat it off. The child was taken to a hospital and the mad cat fled over roofs, mutilating another cat on the way, and escaped, but the mother's terror of its return caused her to vacate her apartments. We do not know how deep and permanent the fear of cats became in mother and child.

Turning now to the phyletic history of the cat, more than fifty species of extinct Felidae, some of them fourteen feet in length, have been unearthed, and fossil cats are found in all the great geographical regions of the earth. They are of comparatively recent and probably not pre-Miocene origin. Some think they originated in North America, where their early remains are most abundant. Cope calls them the most formidable and rapacious of all mammals, and Mivart says they are the best equipped for the struggle of life of all the carnivora. Their jaw used to be long but has shortened and strengthened, and is the most perfect of its size, both for incising and tearing, of all the beasts. The feet are padded for silent, stealthy approach upon their prey, and they have the mechanically most effective of all claws in both curvature and sharpness. Their eye is unique in nature and best fitted of all eyes for both day and night. Their digestive tract is very simple and adapted for the most rapid assimilation and elimination of waste, to keep the body light and agile. Their muscles and their leverage upon the bones are relatively enormous and capable of rapid action of spasmodic intensity as their long, quick leaps show, and the coloring of the wilder species is one of the most protective in the mammal world. Traces of them are found from the very earliest antiquity in the remains of every ancient nation. In Egypt they were worshipped and elaborately mummified. They were domesticated before history began. They played an important rôle in ancient medicine, both in its brews and its superstitions. Chamberlain thinks cats were not domesticated in the ordinary sense and that their use to man has been comparatively slight. Tradition reports that they were often very highly trained to do things that now seem incredible in the way of tricks. They probably came to man as a camp follower and scavenger till women and children took up with them, probably first by nurturing their young. They love and adhere, not to persons, like the dog. but to places. They have changed their nature perhaps less than all the scores of species that de Candolle lists as animals that man has reclaimed from the feral state, that is their psychophysic constitution has been less modified by association with man and they always and everywhere readily revert to the wild state. Their ancestry and nature are solitary, not gregarious like those of the dog. This is reflected in their very deliberate eating habits, formed by feeding alone, and is in sharp contrast with the greedy gobbling of food by dogs, who in the ancient pack had to get their share quickly or starve. When they follow their host from their home it is only for a short distance and with the remnant of the stalking instinct as they follow game or because they hope to be fed, not as the dog follows his master because he has adopted him into his pack. The cat is still the guest of his human host, not because it must be but because life with man is easier and more comfortable. In the history of witchcraft cats have played a great rôle and black cats with glary eyes and bristling back and tail were twin horrors with witches. Cats have been bred in many fancy varieties though they differ far less than breeds of dogs, and now we have not only anti-cruelty laws but cat clubs, homes, refuges and hospitals. Browne gives us a long list of eminent men who were ailurophobiacs and philiacs. Among those with morbid aversions to them were Boswell, Napoleon, Lords Roberts and Raton, the present German Kaiser Wilhelm, and many others, living and dead; while among the cat lovers and petters were Walter Scott, Wordsworth, Hugo, Matthew Arnold, etc. Cats toò have had their own artists and even caricaturists, which a recent author has enumerated.

Children still repeat the ancient behavior of man to cats. (See Hall and Browne: The Cat and the Child. Ped. Sem., 1904, v. 11, p. 329.) As the Egyptians embalmed so children celebrate elaborate funerals of cats with coffins, shroud, burial services, tombstones and even cemeteries such as that which existed of old at Babustas. Children dress them as dolls, babies, put them to bed, draw them in toy wagons, christen, baptize, name them, become physicians to their various and frequent slight or even disgusting ailments and their anthropomorphism causes them to ascribe to them about every human act and sentiment. On the other hand they neglect and often torture them with exquisite cruelty, although they do often develop some sense of responsibility in caring for them, showing sometimes moving affection and sometimes intense if not innate hate and fear of them. Maspero thinks ancient cat worship was not so much adoration of an official cat-god as an expression of the natural admiration of the marvelous psychophysic organization of the cat itself. If it has been an object of religious awe, it has also been bedevilled, for no brew in a witch's caldron was ever without some ingredient from the cat in the recipe for its hell-broth. It its purring is singing, as many children interpret it, its nocturnal waulings are not only sleep destroyers but portents of ill omen. Punch says the cat is the most musical of all creatures because it is full of fiddle strings. Folk lore makes its eyes emit baleful rays in the dark, and a German physiognomist has tried to show the close similarity between the features of the cat and certain human faces. The cat is connected with various demonic personages. If its fur is soft and it loves to tempt us to stroke it and it rubs fondly against our legs, it is because its pelt is infected with both insects and germs, so that it tends to use man's lower extremities as a strigil or back-scratcher. If the play of its young is full of charm, its temper as it matures is uncertain, and few children have not at some time felt its teeth and claws. If it allies itself with man in its warfare on rodents, it fights against him in preying upon chickens and birds. Those who train lions, tigers, leopards and other Felidae assert that in their ability to do things and in their intelligence and disposition they are very like the common house-cat. Their nocturnal courtships and desperate and noisy battles destroy sleep and attract missiles from otherwise even-tempered men. Some are of cleanly and some of innately filthy habits. Their pelt under many conditions shows almost startling electrical phenomena. To call a woman a cat is no less uncomplimentary than to call a man a dog, hound, cur or whelp. The cat is treacherous and also double-natured. The life of pussy playing on the rug or under the stove by day and its carousals by night show its personality to be almost incredibly dual. Statistics show that the cat is predominantly the girl's pet, although the latter after adolescence may prefer the somewhat cat-natured "poodle-lap-dog or puppy," while the dog is the pronounced favorite of the boy. The woman hater, Schopenhauer, said the cat represented the inmost nature of the female sex in both its good and bad qualities. Let us hope not for many reasons, one because so large a proportion of boys are hostile and cruel to cats that it is hard to believe that they are thus developing bad qualities that the other sex will later arouse and suffer from. We will not even repeat Robinet's ungallant inference from his generalization that all misogynists are also cat haters, that the normal woman, if she loves dogs, will love a well-developed and very canine species rather than the poodle variety.

Thus the offspring of the terrible patrofelis or parent of all the cat tribe have been ever since man's advent into the world as man his most formidable brute foe. He has gradually decimated and is likely to exterminate most of the larger and most dangerous species of the family, while taking some of its most diminutive varieties more or less under his protection. The Felidae mnemes and engrams in our psychophysic organization, whatever they are, cannot quite forget the old and long enmity and feud that existed for unnumbered millenia between the Felidae and the Hominidae, and so it comes that occasionally in neurotic and psychotic natures in which ancient traits are conserved, reverted to or hypertrophied, the tiny half-captive specimens that have insinuated themselves into our lives often suffice to reawaken something like the kind and degree of horror which the early founders of the dynasty of man felt for pussy's bigger and more terrible but now retiring relatives. The old fear mechanism in phobiacs has not dwindled proportionately with the size of the members of the feline tribe. The odor, the slithey mode of movement, the various vocalizations, an optical impression of its form or color or the more emphatic stimulus of a scratch or bite, may suffice to activate the ancient fear or indeed only some single symptom or component of it, such as a nervous shudder, the mental imagery of an attack, a faint aversion or some respiratory modification like cat asthma. Thus we cannot hold that all forms of cat dread and aversion must be due to painful personal experiences with individuals of the house species. Social heredity and tradition of pre-historic experiences with the large varieties which may reach back much farther than we think, and hunters' and travelers' tales, and the glimpses of carnivora that children have in menageries certainly have little to do with the juvenile or even adult outcrops of the cat phobias, so that the alternative here seems to be pretty clear between personal experience and the postulation of some form of ancestral traces. We need, of course, to assume not only some generalized temible diathesis but something as specific as the genus Felidae. Those that sensed the big cats most keenly and farthest off, by whatever sense, and reacted to this impression most promptly, strongly and specifically, in a way designed for escape or fighting this particular type of animal, would have the greatest chance of survival, while those dull to signs of these dangers or without discrimination of the kind of instinct with which they had to deal, would be eliminated. Hence it came that these traits favorable to survival were impressed and inbred far too strongly to be effaced in a few hundred years in us all, and it is these residua which chance has preserved in a few exceptional individuals that are touched off into phobias by the slight stimuli that emanate from our dwindled reminders of the age when the cat was in its highest glory in the days of the awful sabre-toothed tiger. The cat family has thus on this hypothesis developed in man a reaction pattern with some quite specific features. Moreover their tread, smell, form, eyes, etc., remain the same. A genus is a combination of highly generalized traits unencumbered by specific and individual ones, and so the psychophysic cat-complex in us in some sense anticipated classification, each being now a voucher or reënforcement of the other. This complex bifurcates naturally enough into two components, one made up of good and the other of bad qualities, and with this also differentiated love and fear of cats, each of which, like all other such affectivities, may now vicariate for, intensify, or pass over into the other.

In fine, the genetic problem which we face here may be thus resuméd. We have a large and compact group of animals much older than man, ranging from the lion and tiger down to the civet cat and mongoose, with form, habit, ability, psychic traits, strikingly similar in all and so unique that they are very distinct from every other genus. Some of these traits or cat categories are their way of stealthily creeping, their sudden spring, their nocturnal life, their remarkable eyes, claws, teeth, their lithe, agile form, their varied vocal gifts, in which they are exceeded only by certain apes, their solitary habits, bristling electric fur, etc. The larger varieties are now rapidly diminishing in numbers and in geographical distribution so that most of us never saw them unless in zoölogical gardens or menageries, although for the by far largest part of human history, down indeed to yesterday on the cosmic clock, they have been distinctly the most formidable of all man's animal foes. The smallest and thus least dangerous genus of the species has lived with man from prehistoric days. Its nature is essentially unchanged by domestication so that it still leads for the most part, the wild life of its feral relatives, and exhibits in miniature every essential one of their traits, to the understanding of which it is in some sense a key. Our children love it and its offspring as pets, and adults for its rat and mice-killing instincts. Both often develop much affection for it, which it never returns. Many if not most children, at some stage of their lives, seem to have some instinctive fear which may focus on any one or more of the above components in its constellation of qualities. Adults sometimes suffer from morbid and excessive cat-fears, far more rarely, though perhaps more acutely, than children. Can these unreasonable fears, so out of proportion to the danger in those who experience them, be explained from actual personal experience with house-cats? Would they be the same in frequency and intensity if all the cat tribe, large and small together, with all the stories and traditions concerning them, had been brought to earth from another planet, say a generation or a century ago? Or again, would the phobias have been the same if the house-cat had always been with man just as it has, but had never had any big relatives, or had they existed only in stories of adventures with them, which we accept on testimony, as most of us do hunters' tales; or again, if the big cats had always existed but always been kept safely in confinement? This, I take it, is the problem of the genetecist here. The most conservative answer we can now make to this question is, I think, that we should suspend judgment awaiting especially more careful analysis of more cases of cat phobias in both young and old, for not a single case has yet been satisfactorily analyzed. If, as the facts so far indicate, these intense fears, especially of children, are often entirely without painful personal experience with house-cats, and if they are prone to focus upon one or more of the above peculiar and salient items in the ensemble of feline qualities, then the least we can assume is that painful experiences of man's phylum with house-cats had left some psychoneural preformation, predisposition, or tendency to *Bahnung* or *Anlage* that could be inherited, and that only a slight stimulus would suffice to bring out in a supervalent degree. Anxiety, which may be defined as *timor quaerens objectum,* finds and seizes upon these latent receptor motor patterns and activates them. If now the old terror of big cats had accentuated these transmitted affective patterns, the causal factor of heredity would be more, and that of individually generated neurotic instability less, so that the ailurophobe would contend with inherited more than with acquired dread, for he would be to some extent reviving the old fear of big cats which he wrongly interprets, from lack of psychoanalysis of himself, as fear of house-cats. These latter are the manifest as opposed to the latent objects of his fears. They are obvious to his consciousness while the absent or extinct monstrous forms are still remembered in Hering's sense below, though not above, the threshold. In what form our cat complex or its specific elements, stealthiness, stary eyes, springing, darkness-haunting, etc., are passed on from generation to generation, we know no more than we do why or how these qualities themselves are transmitted in the cat family, although here I frankly avow my own attitude as not unlike that described by the scholastic phrase, *fides quaerens demonstrationem.* Perhaps the psychoanalysis of my persistent *fides,* if I could make it complete, would itself help toward the demonstration. What constitutes the caticity of the cat? Freienfels, one of the most insightful of geneticists (Zeitsch. f. Psychol., v. 68, 1914), calls it a typical *Stellungsnahme,* an activity disposition and attitude best called a feeling.

The best proof that images of specific objects of fear are not inherited is that fear reactions may be set off by objects that are new not only to the individual but to the race. But it is a shallow psychology, prevalent though it be, that infers that where some stimulus or example of the parent is necessary to arouse fear it is therefore of empirical origin. Even if the cluck of the mother hen is necessary to arouse fear of a hawk in her chick, the reaction apparatus is preformed in the latter and acts effectually upon its first arousal and ever thereafter. So even if a child has to feel pussy's teeth and claws and see its eyes in the dark or to be surprised at its stealth, leap, etc., in order that his fear reactions be aroused, the latter is by no means thus proved to be created by individual experience. Not only is the mechanism preëxistent and acts (whether with absolute spontaneity or needing only a faint stimulus to initiate it) but the arousal of one factor, e. g., claws, probably facilitates the arousal of one or more and perhaps all the others, teeth, stealth, leaping, etc., so that the child soon comes to react to the one differently because of the others. Thus, e. g., the reaction to claws becomes stronger than it would have been if the other fearsome qualities had not been ancestrally associated with and reënforced it, unconscious and neuro-mechanical though this reënforcement be. Thus the recent studies that indicate that goslings reared by hens may not take to water, that kittens brought up alone do not kill mice, etc., does not show that they are born without the instinct and apparatus to do so, but only that in at least some cases a tiny jog is necessary to set it in action, just as the contact of air is needful to start up the respiratory apparatus in newborn babes, so that if they first entered a vacuum and with no change in temperature they would never learn to breathe. Some experimental embryologists now claim that every step of embryonic development is impelled by some outer mechanical, physical or chemical stimulus. But if this be so, the momentum of the whole innate mechanism of heredity is always behind to determine what that next step in the development shall be. Thus the answer to the critics of my view of hereditary fears which have been many and which are best set forth by Varendonck and Stekel is that transmitted fears are imageless, as a few introspectionists now think thought may be, but that they are nevertheless very real. Including the intense but commonly transient form of infantile fear, I estimate that all my returns in the last fifteen years on this subject comprise thousands and that there really may perhaps have been in the world as many phobias as there are nouns in the dictionary. Perhaps every object in the world (in one case the mother's breast) has been an object of unreasonably intense fear. It has even been said that a fear motive is a necessary ingredient for all interests. This itself shows that no specific images of fear objects are transmitted as such but only the complex apparatus of fear response. The house-cat fear apparatus is better developed in us today because of the big cats, and the former have in turn helped to keep the big cat fear from dying out in us. The big ones made a larger place for house-cat fear than the latter could ever have made for themselves, and ailurophobia magnifies the dangerous qualities of the house-cat according to its intensity *as if* up the scale of size. The image of the cat does not swell to leonine dimensions, like the awful poodle in " Faust," but the reaction of the individual to one or more of the cat attributes tends to do just this. Individual experience assembles the cat qualities while racial experience intensifies certain of them beyond normal limits, so that the ailurophobe who always focusses on some trait may in the end readily come to fear the whole cat, though if we had an analysis of many cases, we should doubtless find that they had many different foci and we could then subclassify them by these. The cat image unifies all these traits and so they readily spread by association until they include more or less the *tout ensemble* of cat traits, following thus the same principle as that commonly used in applying names to objects, which first designated some salient attribute—man, e. g., as the upright-stander—but later coming to connote all attributes under the denotation of one.

MacDougall holds that certain birds inherit " immediately conditioned representations of the form of the nest," while to Stout the nest-building tendency is innate in the form of " a rudimentary conation, an actual tendency directed toward an end which is an end to the animal itself and does not merely appear as if it were so to the external observer." In other words, he thinks that meaning in this case is inherited and not merely a connection of blind dispositions. He would not accept von Hartmann's definition of instinct as acting according to a purpose with no knowledge of the purpose but holds that such behavior is more than organically innate. At any rate in all the often indistinguishable blends of born instincts acting for the first time, and experience, the animal always acts from the first *as if* it had an end in view. Some, like Stout, even hold to congenital attention and interest, although no one attempts to say precisely in what form any of these innatenesses are transmitted. I can recall no student of instinct who would not admit that cats inherit a more or less definite attitude toward mice, spiders to flies, hawks to chickens, etc. Are we not just as justified in assuming conversely that mice inherit a definite fear *anlage* toward cats, flies to spiders, chickens to hawks, etc.? Perhaps the victim's orientation towards its enemy may be a little less marked because the cat's impulse to catch mice may have been more developed and more often exercised than that of the mice to escape the cat. There can surely be little doubt in such a case of inheritance of a function, whether or not it can ever be correlated with a definite neural structure and even though we do not know precisely its psychophysic mechanism.

We owe to Freud and his disciples and especially to A. Adler the insight that the preformation of about all psychoses and neuroses is found in childhood and infancy, but we know little of the laws of mutation from their childish up to their adult form. Cases in which an infantile phobia has persisted without change of object up into maturity are rare, while changes perhaps many from one object to another, are the rule, and accident may direct a phobia to a new object. We must assume some similarity between the relation of adulthood and childhood and that between the individual as a whole and the phylum, and the former relation may very often be used as a key to the latter and sometimes *vice versa*. Hence we should expect that the cases in which an ancestral fear has preserved its form in individuals of to-day would be as rare as the persistence of the rudimentary organ which has neither been transformed, eliminated, nor reduced. On the other hand, no one familiar with the facts doubts that a very generic and undifferentiated fearsomeness or *Angst* is inheritable. There must, however, be a scale and perhaps a long one ranging from a *summum gens* of *Angst* down to an *infama species* of phobia, the specific differences, *e. g.*, between the types of cat phobia which may focus now on eyes, now on stealthy tread, or on smell or on leaping, each of which may be a fear fetish unconstellated with the others, and there may be an analogy in the way in which each of these traits reënforces the one that is in the focus and the way in which the relics of the ancient fear of the larger members of the cat tribe reënforces the phobia of pussy on the hearth. We have no good scale on which to measure the more generic and the more specific differences that geneticism needs. Only when we have this can we begin to show in detail in what degree all fears are regressions to childish states and begin to determine the phyletic equivalents of the fear components.

For the neuropathic disposition to know nothing is not to fear nothing, nor to fear all things (for true panphobia does not exist), but to fear without a definite object. Anxiety is not polymorphic but amorphic, and this is the state to which reversion tends and psychoanalysis, no matter from what specific phobia it starts, is not complete until it has traced it back through all its mutable stages to general anxiety. Nor will its genetic exposition be complete until it has described all the stages of its development from this most generic form. To do this we must often have recourse to the phylum and its history. In Adler's laws of compensation we have a new method which will be found hardly less helpful in phyletic than it has already proved to be in the ontogenetic domain.

IX. *Ereuthophobia and its kin.* By far the most formidable enemies men have had have been other men. Hobbes postulated a primeval war of all against all. Everyone, he thought, had a primitive instinct to kill everyone he could. "*Homo lupus homini,*" "the devil take the hindermost," Nietzsche's ruthless will to power, Max Stirne's "I will get, be, do everything to maximize myself, no matter who suffers for pity is a disease," "I and my will, pleasure, passion, are supreme and I will subject everyone I can as an instrument to that end,"—these are modern echoes of a once all too real state. Only later, we are told, did man begin to learn the advantages of association in small and very gradually enlarging groups for the purposes of defense and aggression. Every stranger, and all, indeed, of whose good will man was not well assured, were feared. Mankind was then long composed of small bands of friends tried by and true to each other, and the rest of mankind were mortal foes. The fear which used to isolate these bands was a factor in nomadic life, made our forbears ever watchful for signs of other bands, whose mode of warfare was to steal up unawares, armed with the primitive clubs, points, edges, missiles, and this hostility of man to man was one and perhaps the chief factor that so long kept the human population of the globe from multiplying and made it both sparse and few. Thus down through and after the troglodyte age the world was very scantily peopled. The appearance of a single strange face or an eye staring through the thicket might well cause alarm if not consternation, for the most malign purpose and other hostile hands, armed with the most dreaded weapons, might lurk behind. Those dwelling in forests, caves, and amidst hills would be exposed to more urgent instant alarms of this kind than those in the open prairies, which were perhaps chosen by some tribes to mitigate the ever-present horror of sudden surprise by foes. Thus it was long before the pendulum of compensation began to swing to the opposite extreme and the foundations of hospitality to guests began to be laid.

Babies, true to the law of recapitulation, after the sixth or eighth month, still shudder, fear and turn away from new faces, and only very gradually grow accustomed to strange persons, their looks and ways. The most tactful and insinuating guest has a long task in approaching, touching, giving attractive things, smiling, caressing, wheedling, before the child will come to or perhaps even before it will look his or her way, and in this process are many alternating moods and movements of both approach and retreat. Even in the familiar home circle a fixed, steady stare frightens and the etiquette of adult society taboos staring. Thus little boys tease little girls, and "Teacher, Johnny is looking at me," is a frequent complaint in our returns and many tots grow nervous, fidgety, and perhaps have symptoms if they feel any eye riveted upon them. Even the idea of God's all-seeing eye always watching, and the pictured eye upon the seal of the Odd Fellows sometimes give children marked creeps. Strangers, too, who stop to watch small groups of little children at play may chill their activities, make them self-conscious and often they instinctively drift away to a distance with but slight sense of why they do so. Some country boys in our data, seeing a stranger or even a known adult approaching on a lonely road make on some ostensive pretext a detour through the fields to avoid meeting him from sheer bashfulness. All children have manifold reserves towards new adult acquaintances, however well accredited, until they know them personally. Boys fight new boys and girls criticize new girls. I once visited a large insane asylum on the Bay of Naples and half the patients in the wards made the famous old gesture of the hand with first and fourth finger extended toward me, supposed to ward off the malign influence of my evil eye, and this was kept up until I was well out of sight. For the teacher with a good eye to look steadily at a boy in mischief is a strong deterrent. The glowering of two boys about to fight is one of the most impressive and perhaps determining factors in the whole encounter. The father of seven sons, whenever they quarreled made them stand four feet apart and look each other in the eye, which they generally found impossible to do long and their anger ended in a laugh. Staring each other out of countenance, until one pair of eyes falls or turns, is a contest in which the victory often goes to the strongest will. Looking steadfastly into a patient's eye is one of the most effective old ways of hypnotizing, and Heidenhain thought it so superior to others that he had hundreds of cheap glass eyes, and I once saw him pass down a line of soldiers in Breslau, giving each one of them to hold and fixate, and at the end of a few minutes a large proportion were hypnotic. The eye betrays, and when we gaze into another's eye we seem to penetrate his very soul and conversely feel that others may thus read our inmost secrets. The eye with its concentric curves is the best and most attractive target for other eyes. It goes where attention goes. Young children trying to understand always look not at the mouth but at the eye of the speaker. For even a mother to make big eyes at her baby may cause omatophobia, with a compensatory laugh a little after as the fear is overcome, many of children's interests being the later stages of love casting out fear. The eye has its own language of love, hate and anger as well as of fear, which rhetoric and dramatic art often attempt to systematize.

Physiognomy reveals not only settled character but fleeting moods, making every kind of face possible, as a gymnastics of muscles and moods is sometimes prescribed as a mode of broadening and enriching the emotional life by playing lightly upon every note in its gamut, and we are often told that faces with little power of movement and mimesis go with a limited emotional life. One's face is often one's fortune or disaster. Caricature is often very clever in detecting and magnifying animal traits in human faces. Not only artificial expression, simpers, dimples, frowns, smirks that are sometimes almost grimaces, but often more permanent traits, may be thus intuitively read. Face painting is often made extremely suggestive among savages, while masks have played a great rôle in the history of all the feelings which man inspires in his fellow men. Primitive warriors often make up faces to augment their terror, and superstition thus makes more vivid appeal to the imagination, and art and religion add thus to their effectiveness. Young children often have a stage of fear of teeth (odontophobia), when friends first laugh so as to show an unusual amount of dental surface, screaming at their laugh or smile or if the teeth are gnashed or are shiny or false ones move a little. In the chapter on the development of the sense of self it is shown that children have periods of very special interest in the different parts and organs of their bodies, comparing with others, etc., and that they may have at least transient fear also of large or unusually formed noses and ears or peculiarly tousled or bristling hair. Thus the personal *rapport* which focusses in the eye irradiates to the face, head and even to form, dress and very particularly to movements and gestures, which have a long grammar, syntax, of their own which antedates speech, a part of which is natural and universal and part highly conventionalized. Vocalization, too, has great power to attract and repel children, which adults, despite its influence upon them, which is usually far more than they realize, can hardly comprehend. Long before words are understood, timbre, pitch, cadence, modulation, accent, and speech music generally constitute a language of their own, which becomes less vivid and expressive as speech develops and becomes the language of the intellect, as the above modes of impartation and contagion communicate feelings. To an infant the same sentence may charm and soothe to sleep or excite the most acute fear, according to tempo, loudness, quality, its system of stresses, etc.

Thus, in fine, an individual with all his or her manifold properties of face, body, soul, movement, etc., may be hardly less a supreme object of fear than it may be of love, or in other words, as it is persons we love most so it is persons we fear most, and so it has been since man became man. The old fear and suspicion of those of our fellow-beings not well known or understood still lingers in all who harbor prejudices of race or even color, which are so persistent in us all, or feel aversion to customs, rites, religions, because they are strange and new (so that there are many correlations here with forms of neophobia) and it still lurks beneath the animosities of ranks, classes, parties, and creeds, which hinder the extension of sympathy, charity and philanthropy to cosmic dimensions and complete humanism, and it is these underlying ethnic or national aversions that wars fan into a consuming flame. Here are the deep instinctive bases of sympathy and antipathy.

Partridge[1] published returns from 36 males and 84 females in answer to a questionnaire on blushing (the phenomena as observed in others and flushing as felt by ourselves).[2] There seems little uniformity in the course of the blush. It may first suffuse the cheek and spread, or appear in small spots which fuse, and in some it spreads upward, in others downward, often covering the neck, ears, occasionally the arms, hands, chest, with now and then an alternation from redness to pallor. Blushing in the ear, which is often mentioned from Aristotle to Mosso, which it has been thought indicated close relation between the ear and the heart, may be connected with sudden acumination of sensation to auditory impressions. The causes of blushing in the above data are manifold, such as being spoken to, looked at, making a blunder, embarrassment, and very often the fear of blushing or its mere mention, although none blush so much in the dark. Sometimes tears, weakening, trembling, even of the hands, twitching of the eyes, accompanies it. As to flushing, the preliminary symptoms of it may be tremors, weakness, pressure in the chest, warmth, welling upward, unusual heart action, perhaps dizziness, tingling, numbness, globus, ringing in the ears, pressure in the head, and always self-consciousness and often confusion. Thus the blush is more than the mere local hyperemia which others see and involves usually profound disturbance of the vaso-motor function and vivid emotion. Flushing sensations, too, as distinct from blushing, almost always pass upward. This phenomenon thus means marked change in a distribution of the blood and its pressure, and as the phenomenon subsides it is more normally redistributed between surface and center or parts of the body, and tingling, sweating phenomena occur. The wave or flush of heat very often seems to begin in the chest and this conforms to what is known of the modifications of the mechanisms of circulation. Often stuttering, creepy sensations, cold "tweaks," noises of the ears, involuntary automatisms, and in general weakness, nervousness, occur. Very many girls blush at the course their thoughts take or blush when they are told they are blushing, when they make mistakes, are praised, excited, angry, introduced, talked about, meet people, are mortified, surprised, so that it is difficult to bring all the psychic causes of blushing into a single class. The most generic statement is that we blush when self-feeling is much or suddenly changed. Darwin (probably wrongly) thought the blush spread from the outside to the inside, where it brought confusion, but probably the internal excitation is primary. Some have compared blushing to a mild epilepsy and have especially called the epigastric phenomena cardiac aura. The shock element is always of course more or less involved. Campbell thinks nine-tenths of all blushes are from shyness and also that they are morbid. The only natural blush he thinks is from shame at discovered guilt. Melinand thinks that the chief cause of blushing is when we feel that mental states we wish to conceal are exposed or when others see in us what we desire to hide. Yet many blush not only when their faults and defects are revealed but when they are praised. There can be no doubt that the emotions underlying nearly all blushes are connected with fear. Partridge found that boys of ten or twelve blushed most when teased about certain girls. Blushing surely increases at puberty with the enlargement of emotional life and the inwardization of consciousness. In Oriental marts where girls are sold those who blush bring the highest price, for it is considered in the female a mark of beauty, which cosmetics cannot ape or conceal. Many think that the blush was once very widely diffused over the body and often in savage women it spreads far below the neck so that selection may have narrowed its limits. Darwin thinks it is attention to a part that enhances the capillary activity involved and since the body was clothed the face has been the chief object of attention, and so most now blush there only, and when we are deprecated attention is most drawn to the face. Some think that it was once extended over the whole surface of the body and is a relic of general sex erethism that has drifted away from its origin, and also that sex selection has caused the narrowing of its limits, but why, then, is blushing unpleasant to so many? It is certainly far more common in females than in males of all ages and this strengthens the theory that the shame psychosis was its origin. It may have some deep and not yet understood connection with the origin of self-consciousness itself, as the concealment instincts spread from physical to moral and intellectual spheres.

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[1] Blushing. Ped. Sem., April, 1897, v. 4, pp. 387-94.

[2] A theme which Darwin (Expression of the Emotions), H. Campbell (Morbid Blushing), Wood (Monographs, 1890), Melinand (Chautauquan, 1893), Burgess (Physiology and Mechanism of Blushing, London, 1839), and W. S. Muller (1739) have also treated.

Childhood necessarily involves more or less subordination to others, especially to adults. Children are small, weak and tender, and parents and others are sometimes cruel or do not adapt treatment of them to their degree of even physical development. Grown-ups at best may be formidable and their interests are strange abracadabra to the child. In a sense they are beings living in another world and child life goes on around their legs, sometimes as independently of them as if they were trees. They are bigger and stronger so when they intervene children have to get out of their way or conform. Bigger boys share some of this awe and may be objects of great dread if they are bullies, and older girls know so much and are so wise that they are often formidable. My returns abundantly illustrate all this. One English girl's greatest dread was being taken from the nursery to the drawing-room among grown-ups. Children are repressed in the presence of and often fear grave, solemn people, but still more so those with positive, decided or sharp manners. Many of them feel insignificant and oppressed and perhaps react by some hideously awkward or gawky deed or speech, or even lie in order to assert themselves. Others have a haunting dread of being laughed at and dread to go to places, do things, and would say black is white to escape this. Little girls often fear boys because they may not be kind or would talk about them or even because they are not interested in what they do. Some have a morbid dread of being blamed, censured, of evoking displeasure, and divine what adults like and dread and this shapes all that they do when in their presence. Others would sacrifice anything to gain good will. We have boys who habitually play with girls because they fear the rougher ways of other boys. Some have a morbid fear lest they should even see a fight, still greater lest they should be attacked. Boys often shun wiser or older girls lest they should make fools of themselves, and occasionally they plan long beforehand what they shall do and say. Very many adopt what one or more adults like as their standard in the place of conscience. Some boys are not themselves when they have to meet girls, and we have many fantastic fears of others. Some when alone have a sudden sense that someone is present, turn about quickly, think they see a vanishing shadow, are confident they are not alone. If a door-knob does not turn, some dreadful person must be holding it on the other side. Some children think certain elderly women witches, with dangerous eyes. One girl's phobia was that boys should make hands like claws and claw at her. One laughs and cries hysterically by turns if the eyes of a person or animal are found fixed upon her. Many children of school age if addressed by a stranger blush, cast down their eyes, turn away, bite their fingernails, finger their aprons, and make various automatic movements, sidling away and becoming monosyllabic or perhaps utterly unable to speak or even answer questions. In our lists almost any peculiarity in others may excite fear in children:—black men, if they are not used to them, Gypsies, Chinamen, doctors, ugly, deformed people, ragmen, policemen, preachers, peddlers, and people with any marked and salient peculiarities of physique or character. They dread those with superior manners or those who dominate or dwarf them, have an abnormal dread of the possibilities of criticism. This shrinking instinct is not only obliterated but reacts into its opposite in the impudence of the street gamin. All these fears of others may have blushing as one of their chief traits; sometimes it is immediate, sometimes long delayed. In a few school children it has been so intense as to amount almost to vesication and to actually cause a rash afterward, and it may make children and adults recluses and even compelled to change a vocation. There is a sense of helplessness. The eyes blink, look askance or down, the head sinks, perhaps tears come, there are tinnitus, twitching, often awkward movements, the blusher gravitating towards darkness. It is significant that in some languages the very word for blushing and fear is the same, as in the Swedish "*blygsel.*" With blushing of psychic origin there is always a feeling of inferiority (A. Adler's *Minderwertigkeit* and in his sense). The instinct to avail and be esteemed suffers arrest. Self-esteem is wounded. The standing they aspired to or thought they had attained is set back. Where they thought themselves admirable, they appear ridiculous and perhaps the afflux of blood is the first impulse at Adler's organic compensation. If so, and if it is most often found in psychopaths or those handicapped by inferiorities, it shows that they are corrective impulsions. Yet on the other hand if self-consciousness is the muse of epistemology, why do we blush when the social form of it is suddenly intensified?

Pitres and Régis[1] make three classes of the fear of blushing: 1. Simple fear of it, when the patient thinks of it only while it lasts. 2. Emotional, when he thinks of it at other times but is not obsessed; and 3. True ereuthophobia. In the first case the patient may blush easily but does not care. In the second, emotional type, he is only disturbed. Out of their eight cases seven were men and all were with nervous, alcoholic or tuberculous or blushing inheritance, and themselves neurasthenic, with degenerative stigmata and apparently incurable. These patients often made great efforts to divert their minds from the thought of it, which almost always brought it on, and realized that drinking helped because it naturally reddens the face and so conceals the blushing as well as gives courage, and thus seven of these patients became drunkards. The authors thinks these cases confirm the Lange-James theory because they found conditions modifying blood pressure, such as very cold and hot water, made blushing impossible even when it was thought of, and that when the attack came suddenly there were first the organic impressions, and then the idea. They think the order is first, vaso-motor phenomena, then the emotional confusion, then the fixed idea. They had always blushed easily but it never troubled them until puberty, then it became associated with painful experiences, which developed the fear of it. Vaschide and Marchand[2] experimented upon a man of 38, of bad heredity, enuretic in childhood, later syphilitic, and as a soldier after an attack of dysentery was so obsessed that he went to an asylum. When married could not bear to look at his wife and since her death had never sought women because of his fear. His vasomotor irritability was such that after friction of the skin there was diffused redness. While most of his reflexes were normal, there was delicate trembling of the hands and tongue, delusions of persecution. He would be suddenly seized with the idea that he could not look at anybody without blushing and his character noticeably changed, and there were thoughts of suicide. He shaved himself because he could not endure having the barber look at him, never went to a restaurant, and rarely went out in the day-time. Often in dreams his painful impressions of meeting people arose, and he blushed at the thought of callers. Thus he became more or less nocturnal, turned his back to people and sought courage by drinking. Being told a stranger was coming, the subject felt that he was already there, breathed more slowly and irregularly, with accented respiratory pauses. When the strangers entered there was general uneasiness, perspiration, change of temperature, tremor, especially in the hands, the cheeks grew red, respiration jerky, but he was helped by an elixir of paregoric which made him normal. He showed two types of emotion. First there was the idea that some one would enter and then there was greater amplitude and speed of breathing. When one did enter the breathing was slower, quicker, stronger, irregular, and later he trembled so that the pulse could not be registered. The authors think fear of blushing is of cerebral origin so far as it is an emotion and that the neuro-vascular phenomena are never its cause. The patient changed from one obsession to another, from the fear of blushing to the fear of the power of the drug to prevent it. Claparède[3] summarizes 40 cases, adding one. In these cases in 83% the phobia was found in other members of the family, and 95% were neuropathic. Often there was an original reflex tendency to blush easily, when the ensuing ridicule brought on the phobia. In other cases the phobia seemed first, but in both cases the real phobia did not develop until fear appeared. Thus blushing children are not always phobiac but easily become so at puberty when self-consciousness is heightened. This occurred in 78% of his cases; 62% of them were men and he thinks that while women blush easier man dread to do so more because it is thought to be a sign of cowardice. Melinand[4] gives as the causes of blushing, modesty, shame, timidity and confusion, all referable to the one fear that some one will see what we wish to conceal. For instance, when we are praised we love it and long for more but do not wish others to know our vanity or fear their ridicule if they detect it, and then fancy they detect it and then blush. A young girl hears an improper word, understands and is troubled by it, but wishes to conceal her disturbance because she knows that she is not supposed to understand such words. But she feels others are looking at her, fancies they are guessing her guilty knowledge, and so blushes. The man who suddenly realizes that he is not alone as he had thought fears that he has revealed by a movement or muttered word that which he would not, and so blushes. If this is true, the removal of the fear of concealment would also remove the blush as it in fact does. Lovers do not blush with one another or when alone. Babies up to two or three years do not blush, nor idiots below a certain grade of intelligence. Women and adolescents blush more than men, and adults because they are conscious of more to conceal, while conversely old men blush little and blind less than seeing people. These studies and those of Mosso[5] and Hirschlaff[6] show the correlation of flushing with nutritive disturbance of the organs and the brain, which is compensated by increased effusion of blood most marked in the head, which may contain one-fifth of all the blood, and in a horizontal position any strong feeling makes the head heavier and the limbs lighter. Many change their vocations on account of this habit. It expresses emotional instability and may go with bashfulness, stammering, clumsiness, awkwardness, and makes many a bright child seem stupid. It may rest on an unsatisfied need of freedom. It closes the soul to others, and Dugas thinks, but Hartenburg denies, inclines its victim to be egoistic, idealistic, artistic, a literary man or poet, or in a word a subjectivist generally. Rousseau and Amiel were striking instances. There are usually alternate stages of increase and diminution. Hartenberg[7] discusses the organic basis of ereuthophobia from six cases, in which he finds cardiac erethism, large carotids, low arterial pressure, marked demographism, and tendency to perspire. He notes that the phobia develops as a result of painful social experiences and thinks that where a cure occurs, which is rare, it is generally by bracing treatment.

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[1] L'obsession de la rougeur (Éreuthophobie). Arch. d. Neurol., 1897, v. 3, p. 1-25.

[2] Apropos d'un cas d'ereuthophobie. Rev. de Psychiatrie, N. S., v. 3, 1900, pp. 193-208.

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[3] L'obsession de la rougeur. Arch. de Psychol., 1902, v. 1, pp. 307-334; v. 2, pp. 60-61.

[4] Le sentiment de la peur. La Revue. 1901. v. 39, pp. 526-536.

[5] Fear; tr. from the 5th ed. of the Italian by E. L. Kiesow and F. Kiesow. N. Y., Longmans, Green, 1896. 278 pp.

[6] Über die Furcht der Kinder. Zts. f. päd. Psychol., 1901, v. 3, pp. 296-315; 1902, v. 4, pp. 39-56; 141-156.

Luzenberger[8] first discusses the various names which have been applied to the phenomenon in different languages. In the collective literature of the subject he finds about sixty cases, to which he gives references. The majority of cases occur in men and the phobia seems to be of comparatively rare occurrence, as Janet found only 5 cases in 325 individuals affected with psychasthenic obsessions. It seems to be more intense at puberty and in women also at the menopause. The phenomenology is almost identical in all the cases described. Blushing at some time becomes an embarrassment to the individual. He is affected more promptly and intensely in the presence of strangers or of those who look at him, and is less affected among intimates. The phenomena give him a sense of moral inferiority, of embarrassment and confusion (which is also the case in physiological blushing). In consequence of this anxiety arises. In more advanced cases the anxiety in regard to blushing becomes the predominant feeling. Sometimes this anxious preoccupation becomes so pronounced as to modify the character of the person affected and to render him fearful of appearing in public and he becomes always anxious to conceal his face. As to the mechanism of blushing the author concludes first, that the central reflexes for vaso-motor action are situated in the ganglia of the sympathetic system, and in consequence of this every thought is accompanied by an emotion, and every emotion by a thought in consequence of the connection with the cortical centers. Second, the vaso-paralytic dilation of the face, both in the zoological scale and in the primordial grades of infantile life in the human species, may be an expression of anger, in which case the blushing is the more marked the more marked the anger becomes. Third, that in consequence of acquired social restraints hindering a free vent for this, blushing can arise from repressed anger, thus becoming intimately interwoven in an associative synthesis. Fourth, that shame comes to be joined to blushing in accordance with the degree to which the child has been taught to conceal certain parts of the body and attend to bodily needs in private. Fifth, that therefore in the development of ereuthophobia as a phenomenon of obsession or an obsessional neurosis, the sexual facts of infancy have a great predisposing influence. The author then discusses the various theories of emotions and comes to the conclusion that in emotions whose origin in ontogenesis must always be sought in physical pain or pleasure perceived in early infancy by excitements coming from the non-ego (whether this is referred to the external world or its own viscera), the principal part must be ascribed to the associative function, and this is precisely the reason that its manifestations are so complex. This conclusion in regard to the emotions leads the author to attribute very great importance to inhibitory and dynamogenic associations in the interpretation of mental obsessions, and to accept the Freudian theory of psychoanalysis as the means best calculated for arriving at the true significance of the phenomena.

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[7] La Presse Med. 1911, pp. 140-141.

[8] A proposito dell' ereutofobia; contributo allo studio del mecanismo delle idee fisse e considerazioni psichologiche. Ann. di. Nevrol., 1903. Vol. 21, pp. 267-293.

Soukhanoff[9] gives Hartenberg[10] credit for first characterizing *phobie du regard.* The cases which the latter described do not always blush but may show nearly every symptom of fear, fear of eyes fixed on their face. Bechterew[11] thought this an obsession of timidity, almost always on the basis of degeneracy, with sexual and other anomalies, while Soukhanoff traces it to an "ideo-obsessive constitution." His best case, a young man of 24, found it impossible to think or to fix his attention, became greatly confused in the presence of others, lost his spirits, was scrupulous, indecisive, and all seems to be due to the habit of masturbation formed at the age of 15. Being conscious of obscene thougths, he feared others would detect his unworthiness. In such cases the nervous system is generally impressionable and unstable. It is prone to occur in young people especially in primary juvenile dementia. Règnier[12] concludes that ereuthophobia develops more often a degenerate basis, especially hystero-neurasthenic or other neuroses, and that in this way it develops both greater tenacity and more emphasis. He pleads that medico-legally ereuthophobes should be regarded as persons with only attenuated responsibility because in the crisis of blushing they are liable to commit acts of violence of which they are not conscious.

Fear of being looked at or *Phobie du regard,* may be closely connected with ereuthophobia but is by no means always so. The former may rise from a sense of guilt and especially some consciousness of sex irregularity, abnormality, bodily deformity or ugliness. Its more general form is morbid shyness, which Campbell discusses and thinks common in nervous people and connected with eccentricities; shyness he says is simply "the soul shuddering to feel itself naked" and it does not necessarily involve distrust of self or meekness. It does, however, involve excessive self-consciousness and sensitiveness to the opinion of others. Hartenberg notes that where *phobie du regard* exists it is most painful to have the face looked at and most of all the eye, so that every device to prevent this, such as covering the face with the hands, newspaper, veil, etc., is resorted to. The Empress of Austria, who suffered from this phobia, was never seen without a parasol or umbrella. Bechterew[13] also notes the focussing of this feeling in the eyes and stresses bad heredity as its organic basis and masturbation as a very common determining cause. Sometimes it appears only when unusual exposure of the body is necessary, in others when those who have practised masturbation suddenly realize its evil effects, or think that their badness is betrayed to others. Sometimes being looked at causes strong contraction of the pupils and the patient cannot look at anyone without casting down the eyes, and very often there is some trace of the old superstition of the evil eye. Bechterew gives a few cases in some detail. In some there are many stigmata of degeneration, while in others there seem to be few or none, and the phobia seems to be pretty pure. In others it is associated with various other fears. Its patients can escape its attack by concentration of attention as upon their work. In acute cases they seem to grow silent, distrustful dreamy and idle.

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[9] Phobie du regard. Jour. f. Psychol. u. Neurology, 1906, Bd. 6, pp. 241-247.

[10] La phobie du regard. Arch. de Neurologie, 1904, N. 105, September.

[11] La phobie du regard d'autrui. Médicin russe, 1905, N. 3.

[12] De L'Ereuthophobie. Thesis. Paris, 1896.

Luzenberger[14] analyzed a case of morbid fear of blushing due to shame in a woman who came to him in the fifties. Analysis showed that this really went back to a strange helmenophobia, which dated back to proclivity to worms, which she sometimes used to void. This led her to often look back and even to retrace her steps, lest she had left some such unpleasant trace behind. After she married her anxiety increased and her scruples and habits in this regard made her husband suspicious, and then when she realized the base but unjustified nature of his suspicions, she began to blush until this symptom became fixed and supreme and in her consciousness eclipsed all others. She blushed because her trouble was so secret and because she was reluctant to tell its cause he thought her guilty. It was years after her husband died that she came to the physician with this symptom of ereuthophobia most acute. The author deems this case a complex that is primarily based not on sex shame but on the older, very primitive and even animal instinct to cover excrements. Some primitive races too cover only the anal aperture and not the pudenda and show acute shame only if the former and not the latter are exposed. Luzenberger intimates that the shame that preceded sex shame and perhaps the very first shame was that of one's own excrements, which are universal objects of loathing and disgust, and which must be put out of the way for that reason and because they serve as spoor to expose the individual to his foes, who could thus follow his trail. It was in this domain upon this view that the first blushing arose. Moreover, various primitive races cover the posterior and not the anterior organs and this gives some color to the view that sex shame was derived and secondary. We know that all flushings spread upward and never downward, and on this view blushing in the face is a late development, following the Freudian law, "*von unten nach oben.*" This general view cannot be rightly evaluated without a far more careful study of the way in which human ordure, solid and fluid, has been regarded by mankind in the past and by savages in the present, as set forth in Bourke's "*Scatalogical Rites.*" Here we touch but can only glance at the new vein of study opened by Freudians as anal eroticism. This view of the genesis of shame needs to be correlated not only with the many facts in this field and the study of the sacred rites therewith connected, but with the instinct of which modern hygiene has lately developed a body of knowledge and the new sources of danger, hookworm and others, that center about outhouses, concerning which Stiles, perhaps the best expert, tells us that we Americans are the filthiest people in the world. We have at present here only a vague though extremely interesting suggestion, repulsive as the field is into which it takes us, a suggestion that greatly needs and invites more extended scientific treatment.

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[13] Die Scheu vor fremden Blicke; Centralbl. f. Nervenhk. u. Psychiatr., N. F., 1902, 13:160-167. Also Neurol. Centralbl., 1897, 326-7; 386-9; 985-91.

[14] Zentralbl. f. Psychoanalyse, May-June, 1911, pp. 304-7.

How far is blushing due to old sex fears? To be stared at by men has for ages been for woman a prelude to assault, we are told, and this dread of ancient rapes is transmitted to man blushers. In the modern masher we see only a vestige of its cause. Very different from this is the other sex theory, which regards the blush as attenuated and drifted fragment of sex erethism. If we combine these two we have a factor both of desire and of fear as its root. That both these sex factors do still often enter into the unconscious motivations of it psycholoanalysis seems to leave no doubt. But no one claims that blushers are more sexual than non-blushers, and there can be little doubt that blushing has psychic causes that are non-sexual. Its most generic cause seems to be a sudden change, real or fancied, in the way in which others regard us. A too frank compliment, a sense that we have betrayed something we want to conceal and that our give-away would cause censure or criticism, rob us of some advantage we have over others or violate social convention,—these are its most common causes among adults. If some one we thought a friend or even a lover suddenly stands forth as an enemy, or vice versa, or one we thought neutral suddenly appears as either of the above, we flush inwardly if we do not visibly blush. The ereuthophobe is hypersensitized to such changes of personal amity or animosity even by anticipation. He expects every meeting will involve some change on this scale. His very blushing will cause disparagement because it betrays his anxiety, which he knows to be excessive and would hide. It humiliates him to have it suspected that he is so dependent on the transient good or ill will of others. His flushing may denote the irrigation of the apparatus of defence. We blush when the call of defense toward the good will of others is suddenly lost either way, an insult or attack on the one hand and a declaration of love on the other being the extreme. We love to see others blush, not only because the color is itself aesthetic and also suggests vitality, but also because it shows in a way delightful to our egoism how sensitive others are to our feelings about them. Thus the blusher enhances sometimes exquisitely the self-feeling of the person before whom he blushes, and here teasing reveals one of its chief motivations.

Blushing is a most interesting and ostensive illustration of how gregarious man's relation to his fellow man affects circulation. His dearest wish is esteem, fame, and to maximize himself generally in his human milieu, and his greatest dread is disgrace, social outlawry, and general hate. Flushing is a factor of two variables, first the degree of keenness of consciousness of things to be concealed, and second, a sense that they have been betrayed, the latter of course varying with the kind, amount and suddenness of the betrayal. We are now mortified at moral exposures because we used to blush at the exposure of things of which these are symbols or sublimations. Man's dread of blood freshets has always prompted many mechanisms to check and repress them. Very likely it was once correlated with horripilation and has come in with the loss of hair from man's body, and is most intense where he first lost his hirsute coat. It is a little as if the first bare spots were those where man first felt shame or the first cold spots which needed the warm afflux of blood, although this view is by no means as yet fully made out. It would be an obvious advantage to have the blood go where it was most needed, and this is not now in the skin, for its excessive afflux there leaves the functions and organs that need it unirrigated, if it does not divert it from them, and so causes confusion, weakness, etc. All psychophysic motivations, then, that sent the currents inward and aright have always been a great advantage in the struggle for survival. Depilation and later the development of clothing have vastly reduced the vascular activity of the skin, and made blood freshets more inward and also more effective. When man was naked, and still more when he was hairy, his skin had far greater vascular activity to regulate temperature and also to preserve his hairy coat, and now psychic tensions may revive this ancient vascularity in certain spots and special times by way of reversion. This is not sexual. Moreover sex erethism attracts and does not drive blood away to distant parts. Again, each sex blushes before members of the same sex, and once more sex theorists disagree as to whether sex-flushing means desire or dread. We never blush before gods, animals, or inanimate objects, but only before members of our own species or at thought of them. Finally, there is some reason to think that shame of anal precedes shame of sex acts and functions, as nutrition antedates reproduction. Morbid blushing is a rudimentary phenomenon that goes with hypertrophied self-consciousness before others. It is unpurposive and anti-selective, may be therapeutically reducible by reducing anxiety and over-evolved self-feeling, increasing well-based self-confidence. It does not conform to the Lange-James formula, for we blush because we are ashamed long before we come to be ashamed because we blush.

X. *Pathophobias.* Mental anorexia is loss of the psychic representation or concept of hunger and appetite so that the patient is unable to want to eat, however much he may need to do so. It may begin in grief, in voluntary and perhaps religious fasting, in restriction of diet because of poverty or economy, dietary theories, or very often as a result of medical prescriptions, and, in neuropaths, having started it very readily becomes involuntary, habitual and extreme. It may result in great emaciation or loss of a third, sometimes even half, of the initial body weight in a few months. It is on the bases of such pseudo-dyspepsias or false gastropathies that the many alimentary phobias arise. This state may even come as a reaction to false mental hunger that impels to excessive eating. These phobias may focus on special articles of food or drink and also upon nearly any and every stage in the entire nutritive process. The elective anorexic phobias often begin in the giving up of dishes that the patients believe have disagreed with them, and these phobic obsessions of purely pychic and emotional origin are commonest in young women. Some cannot bear the sight, smell, touch or taste of the tabooed article, or the patient may chew only with the very greatest difficulty and for a long time, trying to get courage to swallow, which perhaps in the end he will not dare to do. There is often excessive or sometimes defective salivation, fear that they may swallow the wrong way or that the mouthful is too large, may contain a bone or some toxic element (a peculiar taste being fancied), that the food will burn or irritate the throat, and when at last they muster courage to try to swallow, there may be a spasm of the oesophagus or pharynx. Thus any incident, a sore throat, some slight unpleasant experience with food, a shock while in the act, a little aerophagic experience, may start the morbid psychic fixation so that the patient comes to react to wholesome food somewhat as a child would to a nauseous pill.

Going down the alimentary canal the false gastropaths, following Dejerines' terms, are always thinking of their stomach and may show about all the semeiology of real stomach troubles by auto- and hetero-suggestion. Any emotion, fear, anger, a sense of sin, that can clutch the throat or affect digestion or the bowels, may upset the stomach and bring nausea and distress that, like the dysphagias, are purely psychopathic. Moreover it causes trouble and effort to get oneself fed, and as appetite, as the Pawlow school have shown, is the chief stimulus to digestion, the stomach balks when it is gone, and there may be regurgitation, flatulence, heartburn, and these experiences cause phobias and obsessions to anchor about the gastric stages of digestion. Foods are classified, eating made very methodic, restrictions increase,—all perhaps that the patient may do better or more work. Every new book or theory of dietetics and every doctor consulted irradiate new suggestions that reënforce and give a more definite form to such fears. One can eat nothing hot, another nothing cold, one only solid, another only liquid food, the vegetarian may acquire a horror of meats, and with fads almost always go phobias. Thus patients often come to restrict their eating more and more (one lady came to only painting her lips with a tiny brush dipped in sweetened water), while with others the restrictions are chiefly qualitative. Of course these phobias may be based upon organic troubles, but in Dejerine's experience three-quarters of all such cases are neuropathic and not organic, and to this his many marvelously speedy and complete cures seem to bear witness. Being of purely psychic origin they only yield to a purely psychic therapy. Trouble, alcoholism, and particularly prescriptions by physicians who do not realize the psychic nature of these troubles, start patients on the downward course and make them conscious of their digestion, and these and their own misinterpretation of their symptoms do the rest. And even gastric atony and distention perhaps to twice the normal size, with all their sequelae of fermentation and auto-intoxication, may follow from purely psychic causes. Again nausea and vomiting may come to result from almost any experience, from slight hyperagousia to the fixed idea that we cannot digest. Thus there are not merely many psychic causes but many degrees of intensity in the working of each, e. g., from slight loss of appetite through mericism and rumination to violent retching, that may be the starting points of phobias, while false cardiopathies, vertigo, etc., add their own quota.

Phobias of diarrhoea often arise from a single accident of self-soiling which in extreme cases may come to control the entire life of the victim, while constipation phobias on the other hand arise most often in patients suffering from hemorrhoids or circulatory troubles who have been warned against obstipation. First comes the state, then the phobia, which latter plays a pathogenic rôle of its own. The discomfort of the process, hurry, laziness, the habit of using enemeta or even rectal suppositories, may inhibit the sense of needing to visit the closet, or patients habitually neglect the moment of inspiration and so the habit with all its evils, nutritive and even sexual, is established, and all these symptoms, if phobias arise either way, are greatly reënforced. Purely psychic enterites and especially of late appendicites have become common and even membraneous enterocolitis with glairy, mucous stools, and perhaps the characteristic false membranes that are usually harmless defences against constipation, become foci of morbid attention, and phobias which may make sensations normally hardly perceptible acutely painful. Curiously enough one visceral phobia may supplant another, the dread which has focussed, e. g., on the bladder changing to the fixed idea of a floating kidney, or the mental representation of polyuria which involves polydypsia (which in some cases have impelled patients to drink as much as thirty quarts a day) passing over to anuria and sitophobia, which may arise from medical advice to patients seeking an anti-fat cure not to drink too much. So, too, frequent or too long delayed micturition may both of them be cerebrally cultivated and take on an obsessive or phobiac character. There are true phobias of urinary incontinence and pollakiuria which is very contagious. The function of the kidneys and of the bladder is peculiarly susceptible to the influence of emotion, and with these are often associated fictive disorders of the prostate, which sometimes develops a phobia itself.

Thus all the functions of the alimentary canal and its annexes which normally work without consciousness are impaired if it invades their domain. The general mechanism is always the same. An accident, slight pain, ailment, the hetero-suggestion calls attention to the part or function which intensely magnifies the sensations arising from them, and makes them often painful and hence awakens a dread of recurrence which may darken into a phobia while the effort to prevent the painful function becomes obsessive. *Why do the false and purely psychic disorders of these, as indeed of all other systems of the body, so closely resemble cases that are organic,* so that even physicians often fatally mistake the former for the latter? Is this fully explained by saying that the patient has heard, read, seen enough of diseases so that he unconsciously simulates the one he fancies he has? Neurotic patients are far more astute than their physicians or even than they themselves know, often acting out syndromes with striking reality so that the methods of distinguishing nervous from organic symptom groups or morbid entities are often quite inadequate. But how can the nervous system reproduce the symptoms of so many real diseases with such striking verisimilitude? Must we not raise the question whether the nervous system, the chief function of which is to record traces of past experience, has among its rich stores of them certain disease patterns more or less individualized, inherited from the long experience of the phylum, which enables even young and ignorant neuropathic girls to reënact the rôles of certain specific diseases intuitively and *a priori* in a way often very like the reality. On this view they act as if they had e. g. real pyloric stenosis or obstruction of the intestine, etc., better than they would if their forbears had not actually suffered from these diseases. The stomach and bowels have completely outgrown these disorders and heredity has left no trace of ancestral experience in them, but the effects they registered upon the nervous system have not been all erased. Attention to a part once the seat of these diseases may awaken echoes or reverberations and so these patients have again the neural part of the disease, the nervous now being primary, whereas originally it was secondary. Moreover still further back all these functions, while they were being originally established, involved a relatively greater psychic factor which also coöperated in developing immunity from diseases and then having done its work so wisely and well, retreated as higher functions absorbed its activities. But in the psychotic and neurotic fancies of these diseases the old dominance of the neuropsychic factor comes back, so that all the phobias represent excessive dread of devolutionary regression, and our revolt is again repeating the old, mostly outgrown pathological conditions during the stages when these processes were being given the autistic perfection which they now have in normal life. Not only may the nervous system and the psyche have diseases in which the rest of the body shares only in a secondary [illegible]rely functional way, but neurotics who either greatly [illegible]e or fear maternity may exhibit about all the symptoms [illegible] pregnancy:—spells of nausea, suppression of courses, gradual enlargement of breasts and abdomen, tympanism, etc., so that physicians are sometimes deceived. Occasionally it comes even to false parturition pains. Of this series of purely psychic phenomena there are all grades, from a few faint transient symptoms to those which are really too long prolonged and intense to be true to fact, and all may be motivated by many kinds and degrees of auto- and hetero-suggestion. Some of these psychogenic cases regulate their diet and regimen, prepare clothing for the expected baby, and in a few very rare cases of delusional insanity not only believe they have experienced all the details of confinement but that they have borne a child that has died or been sequestered, and fiction has even made such an imaginary mother accept a real baby brought in at the critical moment, try to nurse it and rear it as her own, while a dement seemed to accept a big doll and to minister to it for months as if it were really her own, until her psyche, as she showed signs of improvement, managed to have it sicken, die and be buried. Thus neuropathic mental representation may vicariate for real motherhood, sometimes even in chaste and unwed women, so great is the power of the cerebral spinal system and the half-unconscious psyche conserves the ability to reënact racial experience.

Once more, patients who have had and been cured of real diseases often continue to be ill in mind. The spirits, mode of life, and even diet of illness persist after complete convalescence. Their mental and moral life and aptitudes remain those of their period of invalidism. The habits of the disease persist and a perhaps long period of purely functional or imaginary *Abklingen* of these phobiac symptoms, and psychotherapy and reëducation are necessary after they are physically well to complete the mental cure. Perhaps the obsession is grafted on some slight passing real symptom, and the patient seems to really have the disease, which is in fact three-fourths psychogenetic. The real symptom becomes a fixation point and when it has vanished the organ or function continues as at least a *point de repère* of a trouble that at first was only fictive. Such cases must, of course, not be confused with hysterical affections of illness in which the patient so enjoys the luxury of being sick and ministered to or an object of sympathy and tenderness. Probably few if any organic troubles do not have functional complications somewhat proportionate to the neuroticism of the patient grafted upon them, just as conversely imaginary ills may bring real functional disturbances, but the fact that a very slight or a spent disease, which has been recovered from, causes in the psychotic constitution so many syndromes of a purely psychic character that often long outlast and often transcend their cause, points to a facilitization or peculiar predisposition in the psychophysic system due to ancestral experiences with disease, while the verisimiltude of the false to the true cannot all be explained by any Freudian hysterical disposition to take flight from reality into sickness, nor by the tendency of psychopaths to put all they have gathered from reading, doctors, conversation and suggestion into practise. We must rather consider some if not most of these fear-bred symptom complexes as both reënforced and directed as to their character and complications by more or less innate results of racial experience with man's great enemy, disease. Sound though other organs and functions may be, the vestiges of our forbears' distempers are not entirely eradicated from our psychophysic system where they persist latently, ready to become patent upon slight occasion, while in neuro- and psychopaths they easily come to play the leading and even the only rôle. The one most infallible trait which distinguishes their fictitious character from the somatic troubles they simulate is the fact that they yield to correct psychotherapy, in which the vital truth concealed in many crude cults, from Shamanism to Christian Science, New Thought and Emmanuelism is now being revealed in its true light in the field of psychological medicine.

Laryngeal phobias are not uncommon among singers, actors, teachers, orators, etc. Some very slight hoarseness or aphonia that reduces the volume or slightly modifies the timbre of the voice causes such patients to constantly wrap their throats, use pastilles, respirators and their preoccupation with fancied dangers to their voice may bring asynergy or a "phantom voice," all of which of course has nothing to do with hysterical mutism. The state of intense mental interest reduces also both the depth and frequency of respiration until this shallow breathing is compensated for by deep inhalation or sighing. This functional apnoea or reduction of vital capacity thus is from a purely emotional cause which may also even bring about false asthma and short, rapid breathing or polypnoea with sensations of suffocation, which are so often the physiological basis of wild panics or which may result in a settled fear of heart trouble. Again, real thoracic pains may bring a fear that outlasts them. Dejerine makes false pulmones a class of phobiacs by themselves. They interpret even the faintest algias as incipient tuberculosis, grippe, bronchitis, etc., while the precautions of others who have morbid dread of catching cold or of hemorrhages would fill a volume. Some veritable phobias of hiccoughs are recorded.

The heart and circulation, as all know, respond exquisitely to psychic states under the influence of which the heart may slow down toward syncope or rise in extreme cases to nearly thrice its number of beats normal in a state of rest. Palpitation and arhythmic action often rouse phobias. Auscultation, especially if not performed with the greatest precautions, makes settled cardiphobiacs, and if the doctor admits the faintest haemic murmur it is often quite enough to implant a pathogenic fear of valvular lesion or defect. Often the dread starts from some unusual modification of the heart's action by strong and sudden feeling. Among vascular phobias we have not only blushing bnt now a new phobia of arteriosclerosis, if blood pressure or tension changes a little. During puberty heart and blood vessels often grow and otherwise change disproportionately for a time, and our records contain many confessions of young people who have for years watched their hearts, especially on going to sleep, feeling their pulse and dreading sudden death from the balking of the usually most reliable of all organs. Not a few have real symptoms which soon pass away, leaving only a nervous predisposition to excessive fear later on the slightest occasion or suggestion. That the psyche can influence the skin is seen in horripilation, flushing, paling, and even in stigmatization. Phobias of dermal roughness often occur in children, who pick scabs, pimples and try to smooth away every slight roughness of the skin, perhaps by the phyletic momentum of the processes that led to the depilation of man's originally hairy body. A case of actual vesication was reported in Paris in a patient in hypnosis who was told that she was undergoing moxa. Babinski thinks inner tissues may be no less affected trophically, and in vaso-motor and functional ways, by psychic states. Skin phobiacs are always on the look-out for every redness, slight discoloration, pustule or itch, perhaps lest it herald syphilis, or their dread may be fixed upon perspiring too much or not enough, being too hot or too cold, and one with a phobia for drafts is reported to have caught cold from having to sit near a door afterwards found to be a sham one. Neurasthenics also often have an acute dread of fatigue. At the faintest sense of weariness they stop. But fatigue is chiefly in proportion to the consciousness of effort, and patients interested and absorbed in whatever they are doing, are very hard to tire out. One patient unable to hold up her arms three seconds could and did hold them up an hour a day in a far more tiring position to dress her hair. One who could not walk five minutes, walked over an hour talking with the doctor about his troubles. Such patients may be obsessed by the memory of some real fatigue in the past and thus may become phobiac for everything that ever tired them. To do acts with dread causes us to stiffen, tense up, make greater effort, bring in muscles less directly concerned with the act, and if we try too hard in this way to do our best we fail to do even well. This unwilling effort may cause tremors or choreiform movements and sometimes contractures and even paralysis. Some contractures are those of defense which persist in thought long after the occasion which caused them. Thus obsession may fix on almost any part or function of the neuro-muscular system, often even on those portions of it that are not under the control of the will. Some contractures are clearly due to the persistence of thoughts of defense and are cured only when the patient ceases to fear. Other motor disturbances are due to instinctive attempts to correct vicious attitudes. Babinski holds that a special act of the will is necessary to relax a muscle and that it is this act which is lacking in contractures, which may persist in sleep, though rarely in complete narcosis. Some functional paralyses may be due to non-intentional suppression of motor power but all this must be distinguished from hysteria, especially the cultivated professional kinds sometimes developed in old hospital cases who can have acute symptoms of fatigue, chorea, tremors, as well as paralyses and contractures at will or as their doctor suggests. Some of them seem to acquire the power to act out and even to feel about any extreme emotional state they list, and among these pseudo-emotions, probably the most dramatic of them all, fear leads.

The emotions profoundly influence general sensibility. In battle or under the influence of religious excitement, as in the case of martyrs, wounds are not felt, and conversely psychic reënforcement may bring not only diffuse but sharply localized an- or hyper-aesthesias. In psychic cases the dermal topographic disorders due to simulation rarely follow the anatomical distribution of the sensory nerves, and this shows that the absence of pain is more perceptual than sensory. Topalgias too are often visceral and these fixations of the idea of pain start up phobias of disease in the organs focussed on. Optic phobias, too, are of many kinds, and may take the form of dread of fatigue, of strong lights, especially colors, specific diseases, a foreign body in the eye, and also patients may come to think they cannot read without frequent rests, by closing the eyes, they carry glasses of different colors and strengths and change them, fuss with light, shades, perhaps shut themselves in dark rooms, use various lotions, all perhaps psychically caused by applying too much effort and attention to ocular functions that should work unconsciously and automatically. Functional narrowing of the field of vision is often really only the narrowing of the field of visual consciousness. So deafness may be of psychic origin from both lack and excess of attention. When talked or read to repetitions are often called for because anxiety tends to distraction by fixation upon the patient's own bad feelings, or there is morbid irritability to noises from the same cause and over-sensitiveness to drumming and humming in the ear, due to circulation, all of which tend to phobias which are the expression and result of irritable weakness, so common in states of reduced health and cachexia. So in the field of smell, perception and not sensation may be suppressed, and nose-bleed or epistaxis and hyperrhoea may be due to phobiac suggestion the latter sometimes being due only to the mild shock of realizing that the handkerchief is forgotten. Other obsessions are that one breathes better through one nostril than the other, or that both are slightly constricted, while some are haunted by odors. Gustation and appetite too are profoundly modified by general affective states of mind, and food in general or special foods lose their characteristic tastes or it is so modified as to suggest adulterations, unwholesomeness or even poison.

Thus while it would be far beyond bounds to say that there are as many phobias as there are kinds of sensations or perception, the mechanisms exist which might make about every one of the thousands of distinguishable sensations an object of fear and perhaps an expert clinical analyst might safely bet that he could with time and patience enough develop about any sensation into an object of a true phobia in a suggestible and neurotic patient or perhaps in the vast variety of nature somewhere, sometime all sensations have been. Let the sensation or the perception or even its immediate association be made habitually painful and the phobia is inevitable because instinctive and the repressive or defensive mechanism is sure to be set in action. Nowhere does the danger of the interference of consciousness in the operation of reflex automatic and instinctive processes appear more clearly. Necessary and effective as psychoanalysis shows this process of consciencization often to be for re-education, it suggests again the momentous conclusion that consciousness itself is essentially a therapeutic and remedial agency with potencies hitherto undreamed of in this direction because wrongly conceived of. But we must not forget that consciousness always brings dangers also hitherto unknown in its train, for it has strange power to inhibit and to hypertrophy almost every organic, motor and sensory activity. Hence it is that there is always a resistance born of fear in going below the threshold to explore the unconscious and this resistance has many manifestations all the way from the instinct that prompts patients to be reserved to physicians up to the refusal of many psychologists to admit even the reality of unconscious psychic processes.

The point is that about any and every organ or function may become a center of phobiac fixation. One patient comes to fear he cannot go to sleep and regulates his regimen and life during the day in a way which he thinks will favor sleep, developing perhaps many and intricate methods of letting down the psychic tension of the day to the level of the "disinterestedness" which Claparède conceives to be the essence of sleep. He fancies he does not sleep hours enough or not soundly, dreams too much, perhaps modifies the conditions of his sleep, the bed is too hard or soft, the covering too light or heavy, the room too cold or warm, or he may come to rely on eating, drinking, exercise, bathing, rubbing, etc., upon retiring, and the systematization may include phobias of noise and light. Such insomniacs must always find a cause for their wakefulness. Perhaps they fancy they are too tired or ascribe their state to late hours, or are obsessed by a morbid dread of emissions, or of starting or jumping and other spontaneous phenomena. Thus they come to watch themselves go to sleep and pass upon the normality of the process, and while they are coaxing themselves to sleep, on waking introspect very carefully to see if they can recall dreams before they fade, and if they can think their sleep has not rested them. Some are haunted by some specific fear like that of a stroke or heart failure, while a heavy supper and any alimentary disturbance or some slight noise may rouse them suddenly to an acute state of wakefulness and dread. Thus we have phobias of sleep itself and also far more commonly its opposite, the dread of sleeplessness (see section on *pavor nocturnus*). Insomnia also predisposes to every other kind of fear, for it is one of the chief expressions of neurasthenia, and a sense of weakness invites, as a sense of strength and vigor dispels, any and every fear. Fears are indeed inversely as the sense of strength, and rest brings the maximum, and exhaustion the minimum, of fearsomeness, so that the intensity of pet fears is one of the best indexes of the degree of fatigue. Sleeplessness caused the defeat of Jeffries, the pugilist, by the colored champion Johnson, and it has caused endless defeats and panics in war, failures in business, loss of courage and nerve in every strenuous occupation. It is probably the chief cause of "helmet" and other headaches, the hysterical nail, sore-ness in the scalp and spine, intensification or sometimes reduction of reflexes, and it may bring disorders of speech or writing, modifications of the voice and even mutism, paraesthesias, loss of control of attention, self-control generally, and even memory. It sometimes brings a dread of vertigo which may obsess life and a sometimes rather radical readjustment of the relations and proportions of the subjective and the objective. Phobias of madness are commonest in this group but there may also be phobias of committing suicide, as well as of doing outré, criminal and even indecent acts, by a sudden impulse. Such phobiacs are often absent-minded, they cannot collect their ideas, e. g., to write a letter or to keep up an intelligent conversation, and perhaps, especially if they are physicians, they spend hours in examining their pupils, listening to their hearts, testing their sensations, reflexes, while others devise in systematic detail methods of identification in case of death or syncope, prescribe how they should be taken home, treated if they lost consciousness or if some dread disease should suddenly befall them.

Emotion may thus act on the organism very like an infection or intoxication. If convinced that sleep or food do them no good, both really do them less than they would otherwise. If convinced that they breathe badly they may have a phobia of lung tuberculosis, and if any real disease really begins, its symptoms are aggravated and its course accelerated by phobias. If a neurasthenic has any disease he always has also an obsession of it, which may be more dangerous than the disease itself, and as we saw, if the organic trouble is cured its functional manifestations often last on. Phobiac diseases thus are often grafted on to the slightest and most episodic real disturbances and the patient suffers on from memory and continues to nurse himself for a disease which has become purely mental. While emotion and shock may produce many real symptoms, simulation may produce far more, and even other symptoms, and these effects of simulation may be more logical and more like reality than real diseases, being indeed often too true to theoretic type to be true to fact. While the hysterical never worries or fears about symptoms, the neurotic always does so, but here again the best of all methods of testing functional from organic disorders is that of psychotherapy, which may cure the former but not the latter.

ON THE PERSEVERATIVE TENDENCY[1]

By William Silliman Foster

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I. Introductory

The spontaneous appearance of formations in consciousness, that is to say, their appearance without peripheral stimulation or associative connection, is a familiar phenomenon in the psychology of common sense, and a fundamental concept in the metaphysical psychology of Herbart. In the terms *perseveration* and *perseverative tendency* the concept has found continued and increasingly extended use in experimental psychology, since the introduction of these terms in the investigation of memory by Müller and Pilzecker (1900). In the last twenty volumes of the *Zeitschrift für Psychologie* the article on Memory which does not involve one or both of these terms is the exception rather than the rule, and much use is made of them also in the treatment of memory in other journals and in text-books. Not in the field of memory alone, however, but also in such different fields as those of feeling, rhythm, association, action and thought, do we find the appeal to the freely rising or to the persisting mental formation, and at least one writer has based a theory of introspection upon the perseverative tendency.

When an attempt is made to bring together the knowledge of perseveration which has been gained in such investigations, however, great systematic difficulty arises. As the expression "freely rising *or* persisting mental formations" briefly indicates, and as we shall try to show in greater detail in the historical section which follows, no single meaning has attached to the terms. They have, on the contrary, been most frequently used without apology and with a great variety of implications. Where criticisms have been offered, there have resulted numerous different definitions, or in some cases there has come an outright denial of the admissibility of the concepts.

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[1] From the Psychological Laboratory, Cornell University.

While the associative tendency and even the more recently supposed determining tendency have been made the objects of special investigation, and while methods have been developed for establishing their conditions and measuring their strength, we have as yet no direct investigation of the perseverative tendency. For the most part, rather, the perseverative tendency has been invoked to explain results incidentally obtained in attacks on other problems, especially where such results seemed difficult to explain in the better recognized ways. It is in view of this disagreement as to its significance and status that we have undertaken an historical review and direct experimental investigation of the perseverative tendency.

II. Historical[2]

The term perseveration seems to have been first used by the psychiatrist Neisser[3] to indicate the fact of abnormally persistent repetition or continuation of an activity, after the activity had been once begun or recently completed, such as is evidenced in the frequent speaking or writing of a given word or words in unsuitable places. For Neisser the term is descriptive, the name of a symptom to be recognized in the clinic. Later psychiatrists do not limit the term to this use, however. Not merely the externally evident reactions, but "perceptual and purely internal" reactions, ways of apprehending and perceiving things or situations, soon come also to be classified as perseverations. Perseveration takes on an explanatory aspect as well. For some it indicates that "innervation processes which from some cause do not attain their goal or express themselves in one way, do so in another"; for others it means "psychical after-images," or "weakness and slowness of association," and so on. The further uses of the term in psychiatry need hardly concern us. It is enough to point out here that psychiatry offers no single standard content for the concept.

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[2] A review of the psychiatrical literature of perseveration is given by K. Heilbronner, Ueber Haftenbleiden und Stereotypie, *Monatsch. f. Psychiatrie u. Neurologie,* 18, 1905. Erg.-Hft., 293 ff. We are indebted to this review chiefly for the details upon which the generalizations of the first paragraph of our historical discussion are based. On the psychological side are reviews of the literature on memory for certain years by H. J. Watt. *Arch. f. d. ges. Psych.,* 7, 1906, Literaturber., 16 ff.; *ibid.,* 9, 1907, Literaturber., 22 ff., and a critical review by C. Jesinghaus, Zur psychologischen Theorie des Gedächtnisses, *Psychol. Stud.,* 7, 1911, 365 ff.

[3] 65 Sitzung des Vereins deutscher Irrenärtze, *Zeitsch. f. Psychiatrie,* 51, 1894, 1016.

The term perseveration was not common in psychology until after 1900 when, as we have previously indicated, it was introduced by Müller and Pilzecker[4] in connection with the perseverative tendency. The latter term is defined by these authors as follows: "After once having entered consciousness, every idea possesses a perseverative tendency, that is, a tendency to rise freely into consciousness. The tendency generally dies rapidly away. The more intensely attention was directed upon the idea, the stronger is the tendency, and it increases if the given idea or series of ideas is very soon repeated. With cumulative repetition it may easily happen that, at times when the other factors which are besieging consciousness are not of especial strength and persistence (*Nachhaltigkeit*), the given idea or series of ideas will come to consciousness solely in consequence of their perseverative tendency." As proofs of the necessity of assuming the presence of a perseverative tendency, they give a list of observations and experimental results. (1) After one has been busy for several hours in attentive observation of a phenomenon, such for instance as the movements of a pointer over a scale, the phenomenon may suddenly present itself again, in image, with the distinctness of sensory experience, especially at times when attention is not demanded in other directions (Fechner's phenomena of sensory memory).[5] (2) Impressive experiences sometimes reappear in the form of hallucinations, at least at times when attention is not demanded strongly in other directions.[6] (3) A long continued motor activity sets up in the lower centers a tendency automatically to continue or to repeat the activity. After a strong disposition of this sort has been established, the center affected may "reel off" the activity for a certain length of time, without the necessity of further incitement (*motorische Einstellung*).[7] (4) Cases of perseveration, in the sense of the psychiatrists, as described above. (5) Quite against our will, ideas and thoughts in which we have been deeply interested for some time—melodies, chess games, and the like—may keep coming back into our minds. (6) We often make mistakes in reading, hearing, speaking or writing words, where words had in mind previous to the mistakes seem, by their continuance (*Nachklingen*), responsible for the disturbance. (7) In experiments on memory, ideas given as reactions in the first part of a series may recur during the latter part of the series without the volition of the observer, and without any evidence of special reproductive tendencies. (8) In spite of instructions not to think of the series, many observers cannot prevent certain syllables of a series which they have learned from coming to mind in the interval between the learning and the test. (9) In experiments by the method of right associates, not only may a syllable which has caught the observer's attention be given once or oftener as a response in the test of the series of which it was a part, but it may also recur as a response on a number of later days (*habituelle Aushülfesilben*). In cases where the correct associate is partially identical with such a syllable, the chances of this sort of incorrect response are increased. (10) Incorrect responses, in which the syllable spoken is a syllable from the series, but is given in the wrong place, are much more frequent in tests which follow shortly after learning than in tests which follow at longer intervals, even though the number of correct responses in the two cases is approximately the same. (11) Series tested after a short interval give shorter reaction-times than series tested after longer intervals, even though the number of correct replies is approximately equal in the two cases; and mere rereading of recently read material may be accomplished faster than rereading of older material. (12) Mental activity during a period shortly after learning not only disturbs the associative connections (retroactive inhibition), but also brings it about that the syllables are less likely to rise freely into consciousness. (13) Observers differ greatly in respect to the degree in which the above mentioned symptoms of perseverative tendency appear.

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[4] G. E. Müller u. A. Pilzecker. Experimentelle Beiträge zur Lehre vom Gedächtniss. *Zeitsch. f. Psychol.*, Ergbd. 1, 1900, 58 ff.

[5] G. T. Fechner, *Elemente der Psychophysik*, II, 1860, 498 ff.

[6] Fechner, *op. cit.*, 513.

[7] G. E. Müller u. F. Schumann, *Arch. f. d. ges. Physiol.*, 45, 1889, 37 ff. Cf. L. Steffens: *Zeitsch. f. Psychol.*, 23, 1900, 242 ff.

It has often been pointed out that the definition of perseverative tendency given above is not altogether unambiguous.[8] After its entrance (*nach ihrem Auftreten*) into consciousness, and while it is still in consciousness, an idea cannot well be said to have a tendency to rise freely *into* consciousness, but only to have a tendency to continue in consciousness, or to run a temporal course. Only after a formation has disappeared from consciousness is it possible to speak of its tendency to rise freely. It is difficult to say whether Müller and Pilzecker mean that the tendency explains merely the free arousal of the idea, or both this and its continuation. The analogous motor disposition brings about a repetition *or* continuation of the activity; the lower center "reels off" the activity. The syllables in the interval between learning and test may be a continuation of the series as well as a recurrence.[9] It may be the continuation of the series which is damaged by immediately subsequent mental work. The previous word seems to linger (*nachklingen*) in the cases where we make mistakes in speaking; and so on.

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[8] W. Wundt: *Grundzüge d. psysiol. Psychol.*, III, 1911, 572. Jesinghaus, *op. cit.*, 365. W. Poppelreuter, Ueber die Ordnung des Vorstellungsverlaufes, *Arch. f. d. ges. Psychol.*, 25, 1912, 293.

Whether or not these authors mean the perseverative tendency to explain both continuance and free recurrence, however, it is certain that others who use the term later do so understand it. Out of the many cases in which both meanings appear in the work of a single author, we choose two in which the twofold use is expressly stated. Ach[10] says: "The tendency of a psychical experience to rise freely into consciousness *or* to remain (*verharren*) in consciousness as an after-effect has already been established experimentally by Müller and Pilzecker, and called perseverative tendency"; and Ach refers to the memory after-image. Wertheimer[11]

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[9] Müller would probably deny the possibility of such a continuation. In his review of Ebert and Meumann's work on practice in memory (*Zeitsch. f. Psychol.*, 39, 1905, 124) we find: "It is evident at once that in immediate retention it is not a matter of after-images. The after-images of a series of auditory or visual perceptions of syllables or the like would give a simultaneous mixture of sounds or colors, and not a successive appearance of distinct single ideas of syllables, visual signs, and the like." Meumann believes, however, that in immediate recall "reproduction takes place on the basis of an immediate after-effect of the impressions and before these impressions have yet faded out" (E. Ebert and E. Meumann: Ueber einige Grundfragen der Psychologie der Uebungs-Phänomene im Bereiche des Gedächtnisses, *Arch. f. ges. Psychol.*, 4, 1905, 205). One of the observers reports (p. 20) that reproduction followed "almost automatically" on the basis of auditory after-images. Compare the experiments on the range of consciousness, where forty impressions may be present simultaneously according to Wundt (*op. cit.*, 335), apparently without any sign of "mixture."

[10] N. Ach, *Ueber die Willensthätigkeit u. d. Denken*, 1905, 10. (Italics mine.)

[11] M. Wertheimer, Experimentelle Untersuchungen zur Tatbestandsdiagnostik, *Arch. f. d. ges. Psychol.*, 6, 1906, 123. *Cf.* the *nachklingende Bereitschaft* of Pohlmann, *Exp. Beitr. zur Lehre vom Gedächtnis*, 1906, 28.

"understands by perseveration (*a*) the tendency of a psychical content to remain in consciousness, to continue even in the face of an opposing tendency, and (*b*) the tendency which may be ascribed to a psychical content *b* to return into consciousness, not only because *ab*, but because *b* (*xb*), was once present." Some authors, on the other hand, use perseveration to indicate not both, but only one of these tendencies. Jung and Riklin[12] "use the name 'perseveration' in the sense of the investigations of Müller and Pilzecker," and mean by it "*merely the continuance of the previous idea,* in so far as this continuance is manifest in the character of the following reaction." Ebert and Meumann[13] limit the conception in another way. "Perseveration apparently played a very unimportant rôle with our observers. Four men . . . maintained very definitely that outside of the special experimental hours a spontaneous appearance of the material which we had presented to be learned never occurred." Even within a single one of these meanings, we often find further limitations and extensions. Jung and Riklin,[14] for example, use the term merely as classificatory and descriptive; "it is not meant to be explanatory." In their sense, "the concept has no relation to the perseveration in organic brain-processes," although Müller and Pilzecker had quoted just these cases in support of the perseverative tendency. Nor has it any relation to "the hypothetical 'secondary function of the brain-cells,' which for some authors explains the psychological after-effects of the directive idea"; although Müller and Pilzecker[15] insist that the perseverative tendency has a definite influence in holding the mind to one particular course of thought, or in bringing it back to a previous course after an interruption. Jung and Riklin do not count the recurrences of the same reply at different times during an experiment as perseverations, but have a separate category, repetitions, for these reactions. They give, as instances of perseveration, such cases as *melt—hot* followed by *slow—fire,* where, as they say, the conditioned reaction is an association to the previous stimulus-word; although Müller and Pilzecker attribute to perseveration alone those cases solely in which association is not concerned. They take account of perseveration only in case the influence is apparent in the very next reaction after that in which the influencing word is found. In general, we find a large number of instances in which the concepts of perseverative tendency and perseveration are changed from their original sense, and made to fit in with the purposes and convenience of the particular author using the terms.

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[12] C. G. Jung u. F. Riklin, *Diag. Assoziationstudien,* 1906, 31. (Italics mine.) *Cf.* E. Stransky: *Ueber Sprachverwirrtheit,* 1905, 28.

[13] *Op. cit.,* 154.

[14] *Op. cit.,* 31 f.

[15] *Op. cit.,* 75. *Cf.* W. Schäfer, *Ueber die Nachwirkung der Vorstellungen,* 1904.

As we have indicated above, the perseverative tendency is sometimes equated to the "impressional tendency," or to that "readiness" of the idea to be reproduced, which is dependent upon the recency, frequency, etc., with which the impression has been made, and which does not depend upon the strength of its associative connections. With such a conception, the perseverative tendency becomes effective for reproduction only when associative tendencies are also brought into play. This view is held, for example, by Wreschner.[16] "Rather must we say that associative tendencies to reproduction are always added to the perseverative. Certainly the former may be of such little strength or so highly complicated that the single elements are unconscious or, if you prefer, only dimly conscious, and so the appearance of freely aroused ideas may be given. . . . But if I understand correctly the developments made by Müller and Pilzecker, they only maintain the fact that the associative factors do not alone determine the course of ideas, but that along with these factors the perseverative tendencies also have to be taken into account, since they are often present in such a high degree that the associative factors, although never entirely lacking, lose their importance." This also is the way in which Watt[17] conceives the perseverative tendency. "Ideas which have recently been in consciousness have for a certain time the property of coming to consciousness again very easily and quickly, or of being reproduced. . . . This property is called the perseverative tendency of the ideas, and we say that such ideas are in a state of readiness." The question whether or not ideas may rise freely into consciousness, Watt regards as in need of experimental test. The close relation of the perseverative tendency to the impressional tendency is pointed out by Titchener.[18] "Why, then, should we separate impressional and perseverative tendency? Why should we not say, at once, that the impressional tendency varies, oscillates, fluctuates?" And the answer given is that the status of the perseverative tendency (as a sort of rhythm imposed on the impressional tendency, making it now and again effective for reproduction) is uncertain, and that the alleged cases of perseveration are explainable in better recognized ways.

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[16] A. Wreschner, Die Reproduktion und Assoziation von Vorstellungen, *Zeitsch. f. Psychol.*, Ergbd. 3, 1907. 13 f.

[17] H. J. Watt, Experimentelle Beiträge zu einer Theorie des Denkens. *Arch. f. d. ges. Psychol.*, 4, 1905, 341.

[18] E. B. Titchener, *Text-Book of Psychology*, 1910, 400 f.

Perseveration, moreover, has not been limited to ideas and other content-processes, but has been extended to explain the course of consciousness at large. Witasek[19] speaks of the perseveration of feeling. "After it has just disappeared, an act-feeling of definite character establishes a tendency to be aroused again upon another provocation soon after, even though this provocation under other circumstances would not in and of itself be suitable to arouse just that feeling." Stein[20] and other workers in the psychology of testimony speak of the perseveration of feeling-tone. According to some writers not only do we have a perseveration of mental processes, but also of forms of consciousness and of mental acts. One of the clearest and most definite statements of this position is given in the following quotation from the conclusion of Koffka's[21] article on rhythm. "We must ask what it is that perseveres, the form of the groups, the activity, or both together. From our results it follows with certainty that the forms of the groups as such have perseverative tendencies. Such groups persisted, even when the rhythmic experience had ceased, and mere regularity had taken its place. A perseveration of the activity itself is to be recognized when, after a rhythm is once established, a new rhythm is more easily called forth than it would have been without the previous one." Maltzew[22] finds that observers required to judge the pitch of tones or the interval between tones often report a pitch or interval just previously reported, and calls this a perseveration of the pitch or of the interval. Watt[23] speaks of the perseverative tendency of the *Aufgabe;* Ach[24] discusses the perseveration of a determination, as indicated by the continued correctness of reaction without repetition of directions before every experiment, and by the occasional slip into an old disposition when a new or difficult task is set, and the instructions are not thoroughly ingrained in the observer. In this general and more or less descriptive and classificatory sense, we find a very large number of cases in which recourse is had to the term perseveration. Almost as numerous as the users of the term are the sorts of behavior that are regarded as symptomatic of perseveration; the observer in an association-experiment reacts with a previously given stimulus-word or reaction-word, reacts continuously in the same psychological, logical, or grammatical form (rhyme, similarity, opposition, etc.), reacts with a topical sequence of words, and so on.[25]

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[19] S. Witasek, *Grundlinien der Psychologie,* 1908, 341 f.

[20] P. Stein, Tatbestandsdiagnostische Versuche bei Untersuchungsgefangenen, *Zeitsch. f. Psychol.,* 52, 1909, 162. *Cf.* Jung and Riklin: *op. cit.,* Wertheimer, *loc. cit.*

[21] K. Koffka, Experimental-Untersuchungen zur Lehre von Rhythmus. *Zeitsch. f. Psychol.,* 52, 1909, 108.

[22] C. v. Maltzew, Das Erkennen sukzessiv gegebener musikalischer Intervalle in den äusseren Tonregionen. *Zeitsch. f. Psychol.,* 64, 1913, 224 ff.

[23] *Op. cit.,* 343 f.

[24] *Ueber den Willensakt und das Temperament,* 1910, 55 f.

The result of our historical survey, then, is as follows. The terms perseveration and perseverative tendency are used almost interchangeably. They may mean that there is a tendency for ideas once impressed to come to consciousness rhythmically or otherwise, with or without the assistance of the associative tendencies; that ideas once in consciousness tend to remain in consciousness for a certain length of time or to run a temporal course; that certain ideas, from the mere fact that they have been more strongly impressed, are readier to appear in consciousness than are ideas less strongly impressed; that feelings, acts, and dispositions may show the same tendency as ideas in one or more of these senses; and that certain types of physical or 'mental' behavior may be expected under given circumstances. The theory of perseveration, we conclude, derives for the most part from incidental psychiatrical observation and from the psychological investigation of memory and association.[26] The problem of perseveration has not been attacked by itself or for its own sake.[27]

Many explanations have been offered of the facts alleged to prove the necessity of the assumption of perseveration in the sense of spontaneous appearance, and the question as to whether or not the facts can be explained on the basis of the selective or combined action of such factors as mere persistence of peripheral excitation, unrecognized associations, remote associations, place associations, impression, determination, and the like, seems an open one.[28] We hope that the following experiments may contribute towards an answer to this question.

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[25] See Wreschner, *op. cit.*, 11 f., 15, 237 ff.; Jung and Riklin. *op. cit.;* E. Meumann, Intelligenzprüfungen an Kindern der Volkschule, *Exp. Pädagogik,* 1, 1905, 96 f.; R. R. Rusk, Mental Association in Children, *Brit. Jour. Psychol.,* 3, 1910, 365; T. Ziehen, Ideenassoziation des Kindes, *Samml. v. Abhand. aus d. Gebiete der päd. Psychol. u. Physiol.,* 1, Hft. 6, 1898, 25 f.

[26] P. Ephrussi, Experimentelle Beiträage zur Lehre vom Gedachtnis. *Zeitsch. f. Psychol.,* 37, 1904, 56 ff., 161 ff.; W. Jacobs, Ueber das Lernen mit äusserer Lokalisation, *Zeitsch. f. Psychol.,* 45, 1907, 43 ff., 161 ff. Many others might be added to the list of references.

[27] Heilbronner, Stransky, Schäfer, *opp. citt.,* and Poppelreuter, Nachweis der Unzweckmässigkeit der gebräuchlichen Assoziationsexperimente, usw. *Zeitsch. f. Psychol.,* 61, 1912, 1 ff., and Ueber die Ordnung der Vorstallungablaufes, *Arch. f. d. ges. Psychol.,* 25, 1912, 209 ff., touch the problem perhaps more directly than others.

III. Experimental

Preliminary experiments.

In a small number of preliminary experiments an attempt was made to eliminate or to neutralize the effect of associations between nonsense-syllables, while conditions still remained favorable for the induction of strong perseverative tendencies. Under such conditions it was thought that syllables might be spontaneously reproduced, or that they might show their influence in consciousness in some other way. These conditions we tried to secure by the following method.

Five series of six syllables were made up. The syllables were selected by chance from the list in Whipple's *Manual of Mental and Physical Tests* (p. 247); but for the same series no syllable was chosen which, combined with another syllable of the series, would make a sensible word; and no syllable was taken which corresponded with a syllable already chosen in respect to an initial or final consonant. The syllables actually employed were: *dro, zig, luh, ild, tud, pum, aum, rin, orp, ool, sef, nuc, lom, zet, ruj, elt, smi, nen, gla, lel, gur, baw, ime, acq, orm, tob, pru, gos, ith, tas.* The syllables of a series were then written down in the order:

123456 | 352641 | 546132 | 425316 | 214365

In such an order no syllable follows any other given syllable more than once; and no syllable appears to have any special advantage, due to position in the series, over any other.

Four observers took part in the experiments: Miss L. M. Day (D), a graduate student in psychology with four years of psychological training; Mr. A. S. Edwards (E), fellow in psychology; Dr. L. R. Geissler (G), instructor in psychology; and Dr. E. Jacobson (Ja), honorary fellow in psychology.

On a single day, one only of the above described series was read aloud to the observer with an attempt not to introduce rhythm. The writer served as experimenter in this and in all the following experiments. The rate of reading was regulated by a noiseless pendulum set to beat three-quarter seconds, and a syllable was given at each beat with the skip of a single beat between each group of six syllables. The observers were instructed to give equal and best possible attention to all syllables; but they were told that they would not be required to recall the syllables afterward. During the reading the observer, with closed eyes, sat at the end of a couch upon which, as soon as the reading was finished, he lay down. During the following interval of one minute he was instructed not to think of the syllables, but to be as passive as possible and to think of going to sleep, or to attend passively to sensations from the skin and body, if such attention should seem necessary in order to keep his attention from the syllables. At the end of the interval the observer had the task of giving slowly, at a suggested rate of approximately one per second, a list of nonsense-syllables of any character, "just such as came into his head."[29] The experimenter recorded for thirty seconds the syllables given, and took an introspective report of the whole experience in so far as the observer was able to give it.

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[28] See Jesinghaus: *loc. cit.;* Titchener: *loc. cit.;* Wundt: *loc. cit.;* H. Ebbinghaus: *Grundsüge der Psychologie,* 1902, 657 f.

It would seem, theoretically, as if such a method should by the arrangement of the series eliminate definite associations, to a great extent at least, through both associative and reproductive inhibition; while by the factors of strong attention, repetition, recency, small demand upon the attention of the observer in other directions at the time when the list of syllables was being said, and perhaps also by the shortness of the series, the perseverative tendency should still be favored.[30]

The following table shows (1) the number of syllables out of the thirty given in the lists by the various observers, (2) the number of times which such syllables appeared as repetitions, (3) the number of syllables which corresponded to syllables in the series in respect to two letters only, (4) the sums of these numbers (totals of "influenced" syllables), (5) the number of other syllables, and (6) the total number of syllables in the lists.

A study of the Table shows that for all observers except Ja the number of "uninfluenced" syllables is much larger than that of "influenced" syllables. In spite of the fact that the perseverative tendency has been favored by the factors which we have mentioned, all syllables cannot be 'perseverations.' Perhaps the instruction to give a *list* of syllables prejudiced our observers against repetition of syllables. As has been shown by other investigators, learning without the intent to remember, and reproduction without the instruction to give right syllables only, usually result in a small number of "right cases,"[31] though we have little evidence to show whether, under these conditions, associative, perseverative, or "impressional" tendencies are weak.

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[29] Compare the method used by W. Peters, Ueber Ähnlichkeitsassoziation, *Zeitsch. f. Psychol.*, 56, 1910, 167.

[30] Müller and Pilzecker speak (p. 58) of the perseveration not only of an idea but also of a series of ideas, and state (p. 64) that the perseverative tendency of the syllables of a series is still of some strength after five minutes.

TABLE I

	D	E	G	Ja
(1) Stimulus syllables given......	11	10	8	14
(2) Repetitions of stimulus syllables......................	9	13	5	15
(3) Part repetitions..............	29	23	21	31
(4) Total "influenced" list-syllables......................	49	46	34	60
(5) "Uninfluenced" syllables.....	93	113	94	58
(6) Total syllables in list.........	142	159	128	118

A large number of rhymes appear in the lists, and it is due to these in large measure that the numbers of part repetitions [see Table I, line (3)] largely exceed the numbers of stimulus-syllables and repetitions of stimulus-syllables [see lines (1) and (2)].

The observers report that it was not difficult to keep the syllables from coming to mind during the interval. When syllables did come, a probable basis for association was usually given.

D says: "The sounds were absolutely dismissed from mind by moving on the couch. They did not recur. During the rest-period I thought of sleep and said to myself 'Go to sleep.' I tried to keep my eyes fixed toward the window and not turn them towards the experimenter. Once I did turn my eyes involuntarily toward the experimenter and had an indistinct thought of the series, but no definite syllables." "When I lay down, a thought of the syllables came up. (Eyes turned toward the experimenter, vague visual image of the experimenter and table, and kinesthetic feel of the beat of the series.) I looked around and moved eyes before any syllables came."

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[31] See, e. g., Poppelreuter, *Zeitsch. f. Psychol.*, 61, 1912, 1 ff.; A. Aall, Ein neues Gedächtnisgesetz?, *Zeitsch. f. Psychol.*, 66, 1913, 1 ff.

E says: "Felt pressure of eyelids on eyeball (I often use this as a means of getting to sleep), thought of time passing, and thought the time seemed long. No clear ideas, and no syllable came up even indefinitely. Attention on feel of hands on chest, feet hanging over end of couch, etc."

G reports: "Not always a blank as required, because the period was too long. At first attention on drowsy sensations, sort of swimming in head, pressure sensations from face on pillow, noticed ticking of clock, hiss of steam, then a few verbal ideas to which I attended, told myself not to think of words. This brought back two or three syllables in verbal form, *sef* and *dag*. Then another blank—forgotten its contents."

Ja reports: "Persistently increased tonicity of the muscles of the face, twitching of the muscles in the left arm, memory of conversation about a dream which I carried on with G a few minutes ago. Turned my eyes toward experimenter, visual image of him, and then the syllables *zin, lom*. Then I noticed that I was introspecting, clock and steam catch attention, then attention on kinaesthetic sensations in arm and legs."

Introspections of the period during which the list is given show that very frequently the observer voluntarily or involuntarily "thinks of the series," and that syllables belonging to the series are thereby brought into consciousness. Rhyming seems to be dependent, in part at least, upon a partial persistence of the previous syllable. The greater number of "influenced" syllables in the case of Ja may be explained by his report that it is easiest to give a list by sometimes thinking of the series.

D reports: "Consciousness of the *Aufgabe,* setting of my throat, eyes turned toward the experimenter, vague, visual recollection of the series (table and experimenter); looked around and moved eyes, scowled, kinaesthetic verbal image of *l,* started to say it and the rest of the word came of itself; auditory-kinaesthetic images of two or three syllables; said to myself, 'you are making them all from one letter'; remembered that I could do so, but must not do so consciously. Let throat take care of itself unless I felt stuck, then visual image of experimenter and table, *s* came up, and rest of syllable came up of itself; other syllables seemed to follow automatically." "Voluntarily set my throat and it keeps setting itself the same way."

E says: "While I was giving one syllable, the next one often came up auditorily; wondered whether I should think of the syllables read or make an effort not to. They did not seem to come up of themselves, except one—*lom*." "Kinaesthetic set which meant the problem, i. e., to give nonsense-syllables; kinaesthesis in throat when searching for consonants; consonant *l* came up auditorily, and started to speak at the same time; a second syllable came up auditorily as I began to say the first. Sometimes several consonants seemed to be there at once, and one gains its place by the vocal organs following the line of least resistance. The vowels come simply as an automatic filling-in; very often do not get the vowel auditorily until after I have said it. Noticed when I gave *bil* that the *l* was clear in consciousness and began the next syllable."

G says: "Some syllables seem to come of their own accord after a consciousness in which attention is on the sensations from the articulatory organs. Others are suggested by the sound of the previous ones, that is, certain vowels or consonants—chiefly consonants—seem to repeat themselves because the organs are still set for their reproduction. Search for a syllable generally means a kinaesthetic memory-image of the previous vowel, and anticipatory kinaesthetic image of one or two other vowels. The initiatory consonant seems to take care of itself. Sometimes an image of the last consonant of the previous syllable."

Ja. "As soon as E said, 'Say the list,' verbal motor setting and start to speak. Syllables seem to say themselves. I said *blu* and then *plu,* which suggested that I was voluntarily trying to give the series read over again. Then I attempted to let the syllables come of themselves in a greater degree. After a little no syllables formed themselves naturally, and I came back to a thought of the series. This seems to be the easiest way to give a list."

A close scrutiny of the "uninfluenced" syllables shows that a great many of them are very similar. Certain letters are repeated over and over again. Table II will serve to indicate the number of times in which the more frequent initial and final consonants appear in the "uninfluenced" syllables with the different observers. In the case of Ja vowels are given five times as initial and six times as final letters. So great a frequency of particular letters and so great an agreement among observers as to the most frequent initial and final letters can not be the result of chance. Neither is explanation possible on the supposition that certain letters in the series which was read were more frequent or attracted attention more definitely; for the most frequent initial and final letters given by the observers in the "uninfluenced" syllables are not especially frequent as initial and final letters in the series read; and as matter which will be presented later shows, greater attention-compelling power of certain letters is not such as to explain the preponderance.

Certain letters, especially *l, m.* and *n,* are likely to appear as initial letters; and others, such as *l, m, n, r,* and *s* are likely to appear as final letters, when one decides to say something without knowing what one is going to say. Further account of this phenomenon will be given in our discussion of the results obtained in later experiments (p. 420).

The main conclusion to be drawn from these preliminary experiments is that, under conditions such as ours, where the perseverative tendency is intentionally favored, and the associative tendencies are neutralized, there appears little or no evidence of spontaneous reproduction. The apparent influence of the syllables read may be explained in terms of association, impression. determination, and continuance.

TABLE II

Obs.	Total Cases	Initial Letters				
D	93	l 24	m 15	p 12	s 8	r 7
E	113	l 20	z 15	d 14	r 10	k 7
G	94	l 14	m 13	g 12	f 10	s 10
Ja	58	n 9	l 8	t 5	p, z, w 4	
		Final Letters				
D	93	l 26	m 25	p 18	n 7	t 5
E	113	k 25	g 16	m 15	b 9	d 7
G	94	n 28	r 17	m 12	f 9	s 8
Ja	58	b, k, m, p 5				

Variations of the experiments, in which parallel series in the same (not in a mixed) order were learned along with such series for comparison, will be described later (p. 421).

Experiments by the Method of Right Associates.

According to Müller and Pilzecker, the influence of the perseverative tendency is especially evident in the results of the method of right associates.[32] We determined therefore to perform certain experiments by this method. We began with two observers, the D and E of the preliminary experiments, neither of whom had previously worked with this method or was especially well acquainted with the literature in this field. Experiments were made on three alternate days *per* week, and always at the same hour of the morning.

The nonsense-syllables used were adapted from the list 1-60 of Müller and Schumann's twelve-syllable normal series, published by Rupp in 1909. Certain changes were made in these syllables, in order that they should be easier for our observers to pronounce; *sch* was changed to *sh*, *ü* to *au*, and so on, and syllables which had meaning in English were modified. Subsequent series were made up by the experimenter in accordance with Müller and Schumann's rules. The syllables were typewritten and presented step-wise by means of the Spindler and Hoyer memory apparatus.[33] As usual, two blank spaces were left at the end of the series. After preliminary practice, the speed of rotation of the drum was held constant at 8.2 secs. The accented syllables were exposed in the test in the space to the right of the place of presentation. Reaction-times were measured by the Hipp chronoscope, connected in make-to-break series with the exposure apparatus and a Cattell lip-key. The chronoscope was situated in a distant room, and the readings were taken by a second experimenter. Before each experimental hour the chronoscope was tested by means of the large Wundt control-hammer, set at 150σ, and adjustments were made to reduce the mean variation of the reading to 1.5σ or less. The absolute time of the chronoscope reading was found by the use of Wundt's chronograph.[34]. That the chronoscope-times may be regarded for practical purposes as absolute is shown by the fact that ten trials with the Wundt hammer gave 150 ± 1.3σ as measured by the chronoscope, and 149 ± 1.2σ as measured by the chronograph; with a different setting of the control hammer, 180.2 ± 1.3σ with the chronoscope, and 179 ± 0.5σ with the chronograph. Sixteen tests with a large control-pendulum gave with the chronoscope 1758.4 ±1.5σ, and with the chronograph 1757 ± 1.6σ. These figures show that the chronoscope, adjusted for constancy at 150σ, was also reliable for the much longer times which frequently occurred in our experiments.

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[32] *Op. cit.*, 281.

In the first experiments, much as in the earlier experiments of Müller and Pilzecker,[35] the observers were instructed to read the series in trochaic rhythm, giving equal and best possible attention to all syllables, not to think of the syllables during the interval between learning and test, to react in the test as quickly as possible, trying to give the unaccented syllable immediately following the stimulus-syllable in the learning series, but not waiting for absolute conviction of the correctness of a syllable which came to mind without such conviction, and to respond with the word "Nothing" if no syllable came. After the test, which on the average took less than a minute, the observer was required to write as complete an introspection of the course of the experiment as he could. With this requirement it was found possible to complete in an hour two experiments, with a five-minute rest between them.

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[33] Catalogue 21, 1908, No. 215.
[34] *Op cit.*, III, 383 ff.
[35] *Op. cit.*, 8 ff.

Table III shows the quantitative results obtained from 12 series by each of the two observers. The interval between learning and test was three minutes and the number of repetitions was eight. The values were computed as by Müller and Pilzecker (pp. 26 f).

TABLE III

Obs.	*r	Tr	f	Tf	v	Tv
D............	.39	3,290	.42	4,790	.10	16,970
E............	.09	1,480	.83	1,190	.07	1,490

* r = percentage of correct replies; f = percentage of incorrect replies; v = percentage of cases in which no reply was given. Tr, Tf, Tv, are the rounded averages of the reaction times of the correct, incorrect, and no replies, respectively. The percentage of cases in which the reply corresponded to the correct reply in respect to at least two parts out of the three (beginning consonants, vowel or diphthong, end-consonants) may be obtained by 1.00-r-f-v.

The striking difference between the results of the two observers is at once apparent. E gives many less correct replies and many more incorrect replies than D, and his times throughout are very much shorter. Although, as we shall see from the results of later experiments, E's command of a series in learning and his retention and reproduction of it are probably in general considerably less than D's, this fact alone is not sufficient to explain the differences in the above results. The difference is rather due, primarily, to the fact that E evidently laid the greatest stress on that part of the instructions[36] which requires a reaction *as quickly as possible,* and permits reactions *of whose correctness the observer is not certain.* While both the percentages of cases and the times of the right and incorrect responses in the results of D correspond very closely with the values given by Müller and Pilzecker's observer Jahn under the same conditions (.39 to .39,

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[36] Before the work of every hour the instructions were always read to the observer, or read by him from a slip pasted in the note book in which introspections were written.

3290 to 2230, .42 to .38, 4790 to 3200, .10 to .18), E's times fall far below the times both of D and of Jahn. In the early experiments, when judging the correctness of his responses, E would sometimes report: "The syllable is all right, but I and doubtful about its position," meaning that he recognized the syllable as a member of the learned series, but that he could not tell whether or not it was the syllable which had immediately followed the stimulus-syllable. E's attention was specifically called to the fact that no such syllable could properly be called correct or "all right," but no appreciable effect on the number of subsequent right cases appeared. E merely changed the form of his report on correctness. E rarely reported that a syllable came to mind and was rejected; he usually thought about the correctness of a response only after the response had been given.

In learning, E's attention was chiefly on the pronunciation of the syllables, and he paid "much less attention to the way the syllables looked or sounded." He had little difficulty in not thinking of the series during the interval after learning until the experimenter said "Get ready." Then, after E had adjusted the lip-key, he began to "get ready to reply." The following is a characteristic account of this period. "Strong strain sensations. Fixation of the screen. Vague and fleeting visual images,—indistinguishable dark forms on a white ground—a tendency to form the vocal organs." Sometimes during this period images of actual syllables appeared, usually in auditory or auditory-kinaesthetic form. With the experimenter's "Ready," strains increase. The stimulus-syllable is nearly always reported as having been read visually without internal speech. After the perception of the stimulus-syllables, various processes are reported. In a very few cases recognition of the stimulus-syllable occurs, or some other reference is made to the place of the syllable in the learning series. Sometimes a "blank" or a "mass of strain sensations in the head" is reported. Most frequently, however, the perception of the stimulus is at once followed by a "taggy" image of the reply-syllable or of a part of it. These taggy images are about equally often visual and auditory-kinaesthetic. When these images come, E at once starts to speak, developing the syllable "vocally" if only part of it is given in image. Almost as frequently E reports that a syllable or part of a syllable "forms itself mechanically upon the tongue" and "says itself," or the syllable "shoots off automatically." Very often this mechanical reproduction is followed by visual or other imagery of the spoken syllable, which helps the observer to spell what he has spoken and to decide whether the syllable is a true member of the series or not. Sometimes, in the period between successive stimulus-syllables in the same test, E gets ready for the next reaction as he does for the first reaction, and reports images of the learned series or of parts of it; but usually the getting ready consists merely in fixation of the screen and increased strain in the eye-muscles and in the throat and chest.

D very frequently finds the nonsense syllables meaningful, especially in the first few series. Certain syllables were felt to be like 'some-German word," *heil-gaam* suggested a proper name, *zus-shaun* seemed like *süss-schön*, and so on. Certain syllables were more pleasant than others.

D, like E, finds that most attention needs to be given to the pronunciation of the syllables, but unlike E she finds that, after two or three repetitions, she can pay attention to the "looks" of the syllables, can "run over" them visually, sometimes fixating certain letters in turn, and can very frequently anticipate the coming syllable in visual or kinaesthetic imagery. D has considerable difficulty in preventing herself from thinking of the series during the interval, and says she must "keep talking or else think about the experiment." The thought of the series. when it does arise, is reported sometimes as "a visual image of a white strip over there," sometimes as "a white strip on the drum," sometimes as merely "a tendency to turn the eyes towards the exposure-slit." Sometimes in these cases specific syllables appear, in auditory-kinaesthetic or in kinaesthetic imagery. When getting ready for the test, D reports a consciousness of the *Aufgabe,* represented in a number of ways, at different times. Most frequently at first appear verbal images, such as "quickly" and "get that syllable," often with "taggy visual images" of the strip or of a part of the strip on which the syllables are written. Sometimes these taggy images are all that is reported. Sometimes no visual images appear, but D reports "kinaesthesis of eye-movement to the left," or a "kinaesthetic attitude, which means 'get the syllables.'" Quite often, with these representations of the *Aufgabe,* D has auditory or auditory-kinaesthetic (rarely visual) images of specific syllables. Strain sensations and fixation of the screen seem to be less prominent with D than with E.

D generally reads the stimulus-word in internal speech, and sometimes repeats the word once or oftener. Immediately after this D practically always realizes or attempts to realize the place which the stimulus had in the learning series. This realization is characteristically in terms of actual or imagined eye-movement, with or without vague visual images of syllables or dark spots on a white background to the left. "Impulse to turn eyes to the left and down," "hurried eye-movement," "a quick dart of the eyes toward the upper part of the series" are introspections of this period. After this kinaesthetic or kinaesthetic and visual localization, in a large number of cases "taggy" images of the reply-syllable appear in visual or auditory-kinaesthetic form, and the reaction is immediately made. Sometimes, however, as in the case of E, D reports that "the reaction-word said itself as a muscular continuation of reading the stimulus," and sometimes that "there was effort to get something to say (throat-movements)." Sometimes, when no imagery or imagery of only a part of the reply-syllable appears, D replies "by starting to say something and trusting the throat to finish it."

E's reactions, then, if we may be permitted to make the comparison, seem to approach the abbreviated or muscular type; D's the natural or central type. In order to get a more complete and accurate description of all periods of the learning and of the reproduction consciousness, and especially of the processes concerned in the reproduction of the incorrect replies, we determined to favor quick reproduction by definite instructions, to increase the number of our observers, and to carry out a number of purely qualitative experiments before and after the experiments of the main quantitative group. The conditions in this series of experiments were identical with those just reported, save that the four new observers were tested after an interval of one minute instead of after three minutes, and save that here all observers were told to place the chief emphasis upon reacting *as quickly as possible.* They were still to try to give the right associate, but were told not to discard any syllable which came to mind, unless that syllable were felt to be incorrect before a reply could be made. Besides the two original observers, D and E, there took part in this work Dr. H. P. Weld (W), assistant professor of psychology; Mr. C. A. Ruckmich (R), instructor in psychology; Mr. R. C. Holl (H.), and Mr. J. S. Johnston (J), graduate students in psychology. W and J had had previous experience in experiments on memory. Twelve series were given to each observer. The results of the experiments are shown in Table IV.

TABLE IV

3 MINUTE INTERVAL

Obs.	r	Tf	f	Tf	v	Tv
D............	.22	1,360	.58	1,630	.11	2,430
E............	.07	1,390	.70	700	.11	1,490
1 MINUTE INTERVAL						
W............	.21	810	.52	1,430	.21	2,440
R............	.29	750	.56	1,050	.03	2,570
H............	.25	680	.39	1,020	.27	3,470
J............	.06	590	.64	780	.29	3,150

After this group of experiments, the instructions were again changed. The other conditions remained the same, for both groups of observers, as in the previous experiments. The observers were, however, now instructed to take as much time for finding the correct unaccented syllable as seemed to them likely to make for correctness, and not to reply with the first syllable which came to mind unless they felt that it *might* at least be correct. Table V gives the results of twelve series with each of the observers.

TABLE V

3 MINUTE INTERVAL

Obs.	r	Tr	f	Tf	v	Tv
D............	.43	4,610	.42	6,600	.01	9,052
E............	.25	2,970	.58	11,410	.08	19,000
1 MINUTE INTERVAL						
W............	.29	3,200	.41	6,840	.12	14,780
R............	.24	1,130	.50	1,540	.03	4,560
H............	.47	3,330	.39	4,770	.04	8,470
J............	.19	5,610	.60	7,860	.14	18,880

As might perhaps be expected from the fact that he reacted very rapidly in the experiments whose results are given in Table III, E's results in Table IV do not differ greatly from his results in that table. The number of right associates remains approximately the same. There is a slight decrease in the number of incorrect replies, with a corresponding increase in the number of partly right syllables, due perhaps partly to practice in learning. All the reaction-times are somewhat shortened, but Tf still remains less than Tr. With D, however, the change is considerable. Her correct replies are fewer, her incorrect replies are more, and her reaction-times are much shortened. W, R, and H (Table IV) show great similarity of results in the experiments under the instruction to put the emphasis on rapidity of reaction, both with regard to percentages of right and wrong cases, and in the length of reaction-times. These reaction-times are much shorter than those of D, who gives approximately the same *r*-percentage under the same instruction, but who is tested after a longer interval.[37] J, who had considerable difficulty in reading the syllables at the rate of speed at which they were presented, falls far below the other observers of his group in respect to the number of correct replies.

With the instruction to put the emphasis on correctness rather than on speed all of the observers except R, for whom there is a small decrease (more than compensated by the greater number of partly right responses), give more correct replies than with the second instruction, and all save H, who gives an equal number, give fewer incorrect replies. The reaction times without exception show an increase in length.

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[37] *Cf.* Müller and Pilzecker, *op. cit.*, 47 ff.

Before and after the groups of experiments whose results are shown in Tables IV and V, we carried out a number of experiments identical with those of the main groups except for the fact that the experiment was interrupted at a point in its course known beforehand to the observer, e. g., after the learning, or after the first or a following reaction; and an introspection of the period immediately before the interruption was required. On the basis of these introspections, as checked by the introspections given in the main groups, the following attempt is made to characterize the conscious processes of our observers during these periods.

In the learning period, with the instruction to react as quickly as possible, D reports less attention to the visual aspect of the syllables than with the first instruction, and more attention to pronouncing them correctly and emphatically. "Tried to keep my throat set for the first syllable of a pair until the second was in the field, making the accompanying kinaesthesis for the pair continuous. There was, too, something in the eye-kinaesthesis that made the two syllables of a pair group together and separate more definitely from the next pair." "Tried deliberately at first to keep attention on each syllable while it remained in the field, but found that after a few repetitions my throat tended to continue with the second syllable of the pair while the first was still in the field. There was also incipient eye-movement upward toward the coming syllable, and occasionally a hazy anticipatory visual image. . . . Before the last presentation the second syllables of all the pairs but one were correctly anticipated." The syllables less frequently have meaning than before. E's introspections correspond very closely with his introspections for the first group of experiments.

For W learning was "almost entirely a vocal-motor affair." The syllables were "read off" with clear enunciation and strong accentuation. W, like D, also "goes from the final consonant of the first to the beginning of the next syllable of a pair without a break in speech" and tends to "make the syllables (of a pair) like one word." Meanings for the syllables are infrequent and W tries to "inhibit" them.

R pays little attention to the visual side of the experience, but the kinaesthetic and auditory sides are very clear. He reads the series as a whole into one rhythm, accenting most strongly the first and ninth members. His voice drops slowly till the sixth syllable, and then rises till the eleventh, with a drop at the end. The whole series forms "a unitary kinaesthetic-auditory complex." He anticipates the second syllables of the pairs after a few repetitions kinaesthetically, later auditorily also. The series differ greatly for R in their pleasantness or unpleasantness, depending, he maintains, upon all three factors of ease of pronunciation, sounds of the individual syllables and of combinations of syllables, and visual appearance of certain syllables.

H pays attention chiefly to the pronunciation and to the visual appearance of the syllables; after the first few repetitions, more especially to the latter. He anticipates the syllables, for the most part in visual, but sometimes in other terms. The syllables very frequently have sensible meaning for him.

J, as we have before noted, has trouble in reading the series as fast as it is presented, and in the first or second presentations occasionally falls behind and has to skip a syllable or a foot. After the third repetition, however, he begins to pay attention to the looks of the syllables.

Learning under the third instruction differs little from learning under the second, save in the fact that attention for most observers, but especially for D, E, W, and H, is now more than before upon the visual side of the experience.

In the interval between learning and test, D and E report slightly fewer cases in which a thought of the series occurs, than in the first series of experiments. Of all our observers W has the greatest trouble in keeping his mind off the syllables. He made a practice of rising from his chair and walking about the room singing or whistling until called back for the test. R had little trouble after the first few experiments, and J never reported the appearance of a syllable during the interval in any experiment. H reported syllables in about as many cases as did D.

The preparation of D and E for the test in the later experiments is very like that in the first group of experiments. D reports more and stronger strain sensations for the second instruction than for the third, and notes especially the strains in chest and throat, and stronger fixation of the screen. Reference to the learning series often appears, both in the visual and kinaesthetic forms before described and as a general kinaesthetic set, and with this reference syllables sometimes come up in imagery, especially in auditory-kinaesthetic terms. Sometimes the getting ready is reported by E merely as a total bodily set, strain sensations especially in face, chest and throat, strong fixation of the screen.

Visual images of the white strip of paper, and auditory-kinaesthetic images of specific syllables are very frequent with W during the period of getting ready for the test.

R frequently has auditory-kinaesthetic imagery of the series in this period. Thus he reports "a hazy group of auditory imagery which seemed to be a jumble of a number of the nonsense-syllables with no definite order or rhythm. Then some of the syllables were imaged (auditory-motor) together in a series, but I can't now tell which ones." "A number of syllable combinations started to come and seemed to crowd together in the focus of attention."

H usually waits passively for the stimulus to appear in the experiments both of the second and of the third instruction. Sometimes he reports "a visual field with syllables in it as vague, darker lines," but usually he reports that "there is just an empty visual field."

J first "gets set muscularly for a quick reply." Then before him there is a sort of "dark, hazy, slightly transparent background" in which at times certain syllables of the series appear as darker forms to the right of the slot in the apparatus.

E finds no difference in conscious content between reproduction under the second and under the first instruction. D less frequently reports repetition of the stimulus-syllable in internal speech; but practically always, when the reply does not follow as a "muscular continuation of the stimulus," D reports actual or imagined eye-movements to localize the stimulus in the visual scheme which represents the learning series. Very frequently there is a visual or auditory image of the reply-syllable preceding or accompanying utterance.

W most frequently reports that the reply was "purely vocal-motor, without anticipatory imagery." "The reply comes immediately to my throat, or tongue, and is followed by visual imagery of the syllable said." R either reacts in vocal-motor fashion after visual or visual and auditory-kinaesthetic perception of the stimulus, or the reaction is preceded by an image—usually kinaesthetic or auditory-kinaesthetic—of the syllable or of a part of it. H also responds "automatically" sometimes, but for the most part, he reports that the syllable is present in hazy visual imagery before the reply is given. J always reports that the reply-syllable, as a darker form in the visual background, appears before a reply can be made. Under the third instruction the reproduction-consciousness seems much the same as with the previous instruction in the cases of W, R, and J, save that the strain sensations of the preparatory period are less intense and that the processes in the reproduction-period run their course more slowly. For D and H the visual imagery is clearer and more definite, and the reply-syllables are hardly ever given "automatically." Frequently the first syllable which comes is discarded, and a search (either imagined eye-movement or visual imagery) takes place.

When, as in our experiments, a recently learned series is tested by the method of right associates, a large number of the incorrect replies are found to be identical with the syllables of the learning series. Other replies correspond with syllables of the learning series in respect to two letters. Table VI shows the percentages of syllables of the first kind under the rubric frr (false row-right); the percentages of syllables of the second kind under the rubric frrp (p=part). The totals of frr+frrp are shown in the fourth column, and the percentage of the false cases which are either frr or frrp is shown in the fifth column.

By far the greater part of the incorrect replies—on the average about three-fourths of them—are actual or partial reproductions of syllables of the learning series. The small percentage in the case of R under the second instruction is due to the fact that sometimes his reply in this group of experiments was a syllable which, added to the stimulus syllable, made a sensible word.

Especially with the purpose of investigating the relations of the incorrect responses, in their dependence upon the interval between learning and reproduction, a group of experiments was undertaken with two observers, W, and Miss J. N. Curtis (C), scholar in psychology, who had had a good deal of previous practice in experiments on memory. Two sets of experiments of twelve series each, the one with four repetitions of the series and a test after one minute, and the other with twenty-four repetitions and a test after twenty-four hours, were made with C as observer. A set of series

TABLE VI

Instr.	Obs.	frr	frrp	Total	$\frac{\text{frr}+\text{frrp}}{\text{f}}$
Group I.....	D	.29	.10	.39	.93
	E	.20	.30	.50	.60
Group II....	D	.22	.35	.57	.98
	E	.21	.19	.40	.57
	W	.33	.08	.41	.79
	R	.03	.10	.13	.23
	H	.31	.04	.35	.89
	J	.39	.13	.52	.81
Group III...	D	.19	.19	.38	.90
	E	.29	.25	.54	.93
	W	.10	.22	.32	.78
	R	.09	.24	.33	.66
	H	.32	.04	.36	.92
	J	.24	.14	.38	.63

identical with the latter was made with W, who was given one test and afterwards learned one series on each experimental day. C did the same, with the addition of learning and reproducing a series with the shorter intervals between the test of one twenty-four-hour series and the learning of the next. The instructions and the other conditions of learning were the same as in our second group of experiments by the method of right associates, where the emphasis was placed upon reacting as quickly as possible. The reaction-times were recorded in twenty-fifths of seconds, written by a time-marker connected with the Kronecker interrupter, and are expressed in Table VII in tenths of a second.

TABLE VII

	r	Tr	f	Tf	v	Tv
W 24 hours.......	.02	9.0	.73	14.3	.16	25.8
C 24 hours.......	.23	5.7	.30	8.4	.26	11.2
C 1 minute.......	.49	1.6	.22	5.2	.17	7.9

C explained that, on account of past experience in memory experiments, where "ingenious" learning was encouraged, she found it almost impossible to learn nonsense-syllables without attaching sensible meanings to them. She found the attempt to repress such means of memorizing so disturbing that it did not seem advisable to instruct her to do so. To her mode of learning is to be attributed the high percentage of right cases, shown in her results both for the shorter and longer intervals. She reported that most of the syllables of any series had meaning for her in learning, and that these meanings often linked sensibly the accented to the unaccented syllables of a pair. In tests after the one-minute interval, however, these sensible meanings often played no conscious part for C. In general, she reports, first, "an auditory-kinaesthetic image of the stimulus syllable as it sounded in my own voice." Then, if a kinaesthetic or auditory-kinaesthetic image of the reply-syllable does not appear quickly and easily, C "tries to think of the meaning." If still no image of the second syllable appears, the stimulus-syllable is repeated in internal speech a number of times.

In tests after twenty-four hours, C usually reports a perception of the stimulus-syllable as a verbal meaning; then follow various sorts of imagery; usually, however, predominantly visual. An auditory-kinaesthetic image of a reply-syllable usually appears, but is frequently modified in expression, though not in meaning, before it is given.

W learns "in vocal-motor fashion." In the test he repeats the stimulus-syllable over and over, sometimes a reply-syllable is spoken "as if a continuation of speaking the stimulus-syllable," but usually after considerable repetition W decides "to say something" without knowing what he is about to say. He rarely reports during the test a reference to the learning series.

Both observers report cases in which syllables appear outside the experimental hour; but both maintain that in all cases the syllable appears in connection with a thought of the experiment or of the laboratory, usually represented visually.

In our second group of experiments by the method of right associates, when the test took place after a one-minute interval, and where eight repetitions of the series were given, 63% of the incorrect replies given by W were nevertheless syllables of the series. In the tests after twenty-four hours, with twenty-four repetitions, only 4% of his incorrect replies were members of the series. In tests after one minute with four repetitions, 28% of C's incorrect reactions were syllables belonging to the series, while in tests after twenty-four hours with twenty-four repetitions, 20% of these reactions were members of the series.

As we have seen in our historical discussion (p. 396) Müller and Pilzecker consider the fact that in general, in tests of recently learned series, a greater number of the incorrect syllables are identical with syllables of the learning series than in tests of series after longer intervals, as an indication of the greater strength of the perseverative tendencies in the first case. If, instead of perseverative tendencies, we read readiness of the syllables for reproduction, or strength of their impression, the explanation seems as adequate. Certainly when strong secondary associations are present, as in tests which follow soon after learning, the free appearance of syllables cannot be supposed. And if, as is usually maintained,[38] the secondary associations lose in strength relatively more rapidly with time, then it is to be expected that the direct associations will be those more effective after some time has elapsed, and that frr cases will be the exception.

We have found numerous cases in which a particular syllable is given as a reply several times in the same test and in the tests on different days; but the explanation can, at least in many cases, be made without supposition of free arousal. D, for instance, gave *maan* as an incorrect reply in experiment 3 with the report: "Visual perception of the stimulus; eye-movement to place it in the series, but no syllable came; eye-movements continue over place of series, then suddenly visual image of *maan* (the spelling of *maan* had made a distinct impression on me in learning). Said it at once." This is an instance of an frr syllable aroused through a place association. In experiment 9 D gives the correct reply *maun* with the report: "The stimulus-word *hos* made a strong impression on me (in learning) as the first syllable of a person's name, and the second syllable as meaning *man* was connected with it; pleasant organic sensations. Now with perception of *hos* and recognition of its place in the series, in terms of eye-movement and visual image of the exposure field, the same organics as before. Set my throat, a lot of eye-movement; then *maun* came out in a great hurry." In the experiments under instruction to react quickly, 13-24, *maan* appears twice as an incorrect reply, *maal* three times, *man* once and *mmm* . ., an uncompleted reply, once. In every case D reports that after the stimulus-syllable nothing would come; then occurred a hurried eye-movement over the place of the series and a setting of the throat, which finally is released by utterance. In one case a visual and in one case an auditory-kinaesthetic image of the syllable is reported as preceding utterance. Not until experiment 24 did D realize that a similar syllable had previously been given as a reply. D, then, especially when no syllable will come visually, sets her throat to say something. With this setting of the throat the saying of a syllable beginning with *m* has become strongly associated, and is now realized in the absence of other associations. W gives as incorrect replies, under the instruction to reply quickly, *neuf* twice, *neb* twice, *nez* once, *nef* once, *nev* once, *nif* once, and other syllables beginning with *n* and *m* once each. Under the third instruction he gives *mal* three times, *ner* twice, *nur* twice, *neuf* twice, and other syllables such as *nar, nal,* once each. In practically all such cases W reports strain in the throat, hesitation, or a blank, "I am stuck" feeling. In the incorrect replies of the other observers similar syllables are given when the observers are at a loss for the reply and "set their throats to say something."

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[38] Ebbinghaus, *op. cit.*, p. 647.

It is noteworthy that all the five *habituelle Aushülfesilben nöf, laf, noch, mön* and *meun,* listed by Müller and Pilzecker (p. 62), begin with *l, m,* or *n,* the letters which we found to occur so frequently as initial letters in our experiments by the method of right associates, and in the "uninfluenced" reactions in the preliminary experiment. With the method of right associates in 207 reactions, in which the incorrect response did not correspond even in respect to two letters with the syllable of the series, the frequency of the initial letter was: n, 48; m, 35; l, 22; t, 19; r, 13. The frequency of the final letters was: m, 28; l, 26; r, 26; f, 18; s, 14. By chance, each letter should occur about ten times. If we arrange the total number of letters, substituted for the correct letters in the reactions where a syllable is given which corresponds with a syllable in the series in respect to two letters, we find the following relations of frequency. For 183 initial letters: n, 32; t, 28; m, 25; l, 20; d, 20. For 171 final letters: r, 31; l, 28; s, 27; m, 22; f, 20. By chance, each letter would occur here about nine times. Observers seem to differ very little from one another in respect to the letter most frequently given in these cases, but the smallness of the number of cases prevents us from drawing more definite conclusions. Certain experiments without the lip-key and other experiments, in which the Rousselot microphone was used, show that the form of key or the absence of any key makes no appreciable difference in the character of the response, so far as the predominance of these letters is concerned. These same initial and final letters appear frequently in the totally wrong cases in tests of series of syllables in which the letters themselves do not appear, and even with observers unaccustomed to work with nonsense syllables. It does not seem, then, as if these *habituelle Aushülfesilben* were necessarily recurrences of syllables recently attended to.[39]

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[39] The letters, which, from their frequency in the right cases, appear to have an especial attraction for attention, are not by any means in our experiments the same letters as those appearing most frequently in the *Aushülfesilben*. *Cf.* Müller and Pilzecker, *op. cit.*, pp. 254 ff.

Experiments by the Method of Retained Members.

A series of experiments by the method of retained members in which the method somewhat resembled that of our preliminary experiments, was also performed.

With the exceptions mentioned below all conditions of experimentation were the same as in the experiments by the method of right associates. On each day a twelve-syllable series of nonsense-syllables was presented for eight repetitions and read by the observer precisely as in the method of right associates (repeated series). After an interval of one minute the observer gave as many of the syllables as he could recall in 45 seconds, reacting with the lip-key. Alternately before and after this series a second twelve-syllable series was presented and tested. In the successive presentations of this second series (mixed series), as in our preliminary experiments, the order of the syllables was so changed that no single syllable was ever in the eight presentations followed by the same syllable as in any previous presentation. The syllables were so arranged also that no syllable had any special advantage over any other by reason of its position (first or last in the presentation) or by reason of accentuation (first or last in a pair), and two different arrangements were used still further to decrease the chances of advantage of syllables by place in the series. By this arrangement strong direct associations should be avoided. The syllables of this second form were typewritten on a long strip of kymograph paper, the eight arrangements following one another with the customary two blank spaces between presentations. The actual exposure was made by the Spindler and Hoyer apparatus, the strip being rolled as a band over a second light drum at the back of the apparatus. The times of the reactions were recorded graphically on a Ludwig-Baltzar kymograph, and the actual times recorded alongside of these in twenty-fifths of a second by a marker connected with the Kronecker interrupter. After preliminary practice the observers found little difficulty in using the lip-key for successive reactions, though in a few cases the observer believed that the second reaction may have been slightly delayed by the necessity of closing the key after the first reaction. After a short preliminary practice, 12 experiments with each sort of syllable order were made with all of the four observers W, R, H, and J. The observers were told to give not only such syllables as they were sure belonged to the learning series, but also other syllables which occurred to them, if they believed that these other syllables might possibly be correct.

TABLE VIII

Obs.	Total Reactions		Right Reactions		Incorrect Reactions	
	Rep.*	Mix.†	Rep.*	Mix.†	Rep.*	Mix.†
W	79	74	.51	.55	.18	.27
R	103	95	.46	.60	.32	.21
H	88	81	.83	.77	.05	.09
J	66	66	.59	.52	.06	.17

* Rep.= Repeated order. † Mix.= Mixed order.

As is evident at once from the above Table the percentage of correct replies after an interval of one minute is not directly dependent upon the fact that the direct associations are repeated and strengthened in learning, for the percentage of right reactions is nearly as great (H, J), or even greater (W, R.), when no such repetition occurs, as is the case in the mixed order. The rather surprising fact that W and R give slightly more correct syllables and that H and J give almost as many correct syllables in the series with the mixed order, as in the other series where the direct associations are repeated eight times, is undoubtedly to be explained by strong indirect associations and by strong impression. Three of the observers report that the individual syllables in the mixed order stand out more clearly in attention. In the mixed order the observers do not know, when one syllable is in the field, what the next syllable will be, as they very often do in the repeated order. The consequence of this unpreparedness is that the observer's attention to the presented syllable is always demanded in a high degree. All observers report that more attention has to be paid to the visual appearance of the syllables in the mixed order. That direct associations are not of primary importance for recall of a very recently learned series is also shown by the fact that the usual order of recall of syllables with the repeated order of presentation is not the order of the syllables in the learning series. The first syllable to be recalled is two-thirds as often the eleventh of the learning series as the first, and the other syllables follow in very irregular order. Still another fact that tends to show the same thing is that out of 48 cases in which a correct syllable is followed by a correct syllable in the reproduction of the series with repeated order, R gives the directly associated syllable only 11 times; W gives it in only

10 cases out of 49; H in only 21 cases out of 70; and J in 9 cases out of 48. The number of times in which the following syllable in these cases is the syllable immediately preceding the given syllable in learning for R is 9, for W is 4, for H is 11, and for J is 6.

The average length of time elapsing after the experimenter's "Now" and before the observer's first, second, third reaction and so on, is given in the following Table. No averages are calculated for those places in which less than six reactions appeared, and the times which were measured in twenty-fifths are here expressed by the nearest tenth of a second.

TABLE IX

OBSERVER W

Place	1	2	3	4	5	6	7	8	9
Time: rep................	3.9	6.8	8.0	9.9	15.9	22.0	31.3	35.4	....
Time: mix................	2.4	5.4	7.9	11.9	22.2	28.9	34.3	....	....

OBSERVER R

Place	1	2	3	4	5	6	7	8	9	10
Time: rep............	1.3	2.2	3.7	6.3	9.7	17.1	23.3	24.3	32.6	34.4
Time: mix...........	1.2	2.6	4.6	6.9	12.1	17.4	26.1	34.7	39.8	....

OBSERVER H

Place	1	2	3	4	5	6	7	8	9
Time: rep................	1.9	4.1	7.4	11.2	17.1	18.2	20.1	26.7	29.6
Time: mix................	1.7	3.9	5.8	9.4	18.1	23.9	27 4	28.6	....

OBSERVER J

Place	1	2	3	4	5	6	7	8	9
Time: rep................	2.9	5.3	7.9	9.2	10.6	14.2	26.5	....	....
Time: mix................	2.1	3.5	5.9	9.5	12.8	20.3	31.3	....	....

In the recall of the series with repeated order, the observers give the first or first few reactions later on the average, and the following reactions sooner on the average, than in the series with mixed order. The difference is especially great in the case of W, and can at least in his case be explained on the basis of his introspections. "With the short [*i. e.,* the rep. order] series it takes time to readjust, to put myself into the set of the learning series. I start to *visualize* the series." "At 'now' very brief time in which I readjusted. Blank; visual field dark, visual image of syllables." "This reproduction is very different from the reproduction in the long [*i. e.,* mixed order] series. There I was waiting for a release; here there is a real recall." In general, then, in the repeated series W tries to get back into the learning situation and to visualize the syllables. In the mixed series, however, the first syllables follow in vocal-motor fashion a set of the throat. After the first three or four syllables, usually comes a time during which no syllables are given. W reports: "Relief, passivity, attention on nothing in particular." Then W tries to get the syllables visually or repeats over in internal speech the syllables already spoken. A similar state of affairs is also reported by H. R reports that reproduction follows the "now" in autditory-kinaesthetic terms in both cases; reproduction for J is usually in visual terms. The shorter time of the first replies in the mixed series may depend upon *vocal-motor* readiness, due to higher attention. The increased time in the later replies is explained by the fact that the direct and the place associations in this form of series are weak. In the repeated series place and direct associations continuously aid the observers.

The incorrect replies of this method are distributed almost entirely in the last part of the recall-period. A large number of them are very similar, and resemble the *habituelle Aushülfesilben* of the method of right associates. With W, for instance, *nev* is given as an incorrect reply five times before the series of experiments is two-thirds finished, and not until that time does he realize that it has been given before. When the fact is noticed, he remarks: "I think this syllable has come up a number of times before. I don't think it is in to-day's series at all. It came when I was balked; I felt helpless, my throat was tense, I was waiting for something to come, and my face and eyes were screwed up. Feeling of emptiness. Determined to say something and *nev* was uttered. Then I saw it in type, not black, but reddish." After this report the syllable did not appear again in the series. R and J also report that such syllables are uttered after the throat is set in an attempt to say something.

IV. CONCLUSION

In the historical section of this paper, we pointed out that the concept of perseveration has acquired a wide variety of meanings. Perseveration may be the designation of at least four psychologically distinguishable states of affairs. (1) It may mean that a content-process has a tendency to run a temporal course, to have a *continued* existence. Since this meaning is implied in the very term "process," it seems useless to adopt a special term for designating it. (2) Perseveration may mean that a content-process, having once appeared in consciousness, does not at once disappear from consciousness, but stays or remains *in* consciousness for a certain time without reference to a change in its durative character.[40] (3) Perseveration may mean that a given mental formation which has once been in consciousness is likely to appear in consciousness again. (4) Perseveration may mean that, if a given mental formation has once been in consciousness, its recurrence need not be attributed to any condition save this of having once been in consciousness.

It is the factual justification for this latter concept that our experimental work has called into question. In our experiments we have met no facts which seem to make necessary the supposition of a tendency for an idea to arise without the probable coöperation of associative tendencies. When a syllable-idea came to the mind of our observers, in the interval between the learning and testing of series, or in the intervals outside of the experimental hour, the observers reported that such an idea followed or accompanied an idea of a larger situation of which it formed a part. In the "natural" rhyming of syllables, which occurs when the problem of relatively unrestricted speaking of a list of nonsense-syllable is given, our observers find cases in which the previous sound is still in consciousness when the new syllable arises. We have maintained that the syllables which are habitually given as reactions by observers, in the method of right associates, when the observer is at a loss for vocal-motor expression, are in many cases not syllables which have been previously learned, but are rather natural reactions, due merely to the intention to utter *some* nonsense syllable; and we believe that the recurrence of such replies may be sometimes partly explained by an association of the given motor impulses to the conscious processes characteristic of the state. The greater number of false cases in the tests of recently learned syllables is explicable, we believe, by the assumption that indirect or remote associations lose relatively much more rapidly in strength than do direct assiciations, and that, as more and more repetitions are given to a series, the repetitions become more and more effective for the direct and less and less effective for the indirect associations within the series, through the partial inhibition of the latter by the former. In a series, therefore, in which direct associations are equal to those in another more recently learned series through the advantage of more frequent repetition, the remote associations will be much weaker, and in it the number of false cases which are syllables of the series will be less.

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[40] Jesinghaus, *op. cit.*, 366.

The shorter reaction-times of right syllables in recently learned series, whose percentage of right cases is equal to that of another less recently learned series, may be explained as readily on the basis of strong impressional tendencies or greater readiness of the syllables, as by postulating perseverative tendencies.

The non-experimental observations which have been urged in support of the theory of a free arousal of ideas or feelings and the continuance or recurrence of a given form of mental activity, may be explained, on the basis of some or all of the following factors; unnoticed motives to association, such, for instance, as kinaesthetic or organic sensations; strong impression or determination; remote associations; place associations, and the like. Many alleged instances of perseveration may be explained as the mere continuance of the given process instead of its re-arousal.

Finally, conditions which are supposed to favor perseveration do not always bring about a reproduction. We may have been *recently* awakened from a *vivid* dream, and we may *try* in vain to remember the ideas of the state, whereas an idea associated to a dream idea will often accidentally bring up that idea without our endeavor to remember. The stimulus associated to an action in the patient's mind by the operator in hypnosis may touch off the suggested but forgotten action at a later time, even though the action be foolish.

Not only may the necessity of supposing a spontaneous recurrence of mental contents be questioned, but a spontaneous activity in the nervous system, which seems thereby to be implied, is also made improbable by physiological facts so far known.[41]

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[41] See Wundt, *op. cit.*, 572 f.; Jesinghaus, *loc. cit.;* Poppelreuter, *Arch. f. d. ges. Psych.*, 25, 1912, 293 ff.; Titchener, *loc. cit.*

AN HISTORICAL NOTE ON THE JAMES-LANGE THEORY OF EMOTION

By E. B. Titchener

In his article of 1884, James tells us that his view of emotion derives from self-observation: "fragmentary introspective observations" had "combined into a theory." In the *Psychology* he remarks: "Prof. C. Lange, of Copenhagen, . . . published in 1885 a physiological theory of their [the emotions'] constitution and conditioning, which I had already broached the previous year in an article in Mind."[1] Lange, like James, announces this theory as a new discovery. The motive and method of his investigation were, however, different from James'; he worked, in the interest of practical medicine, from the side of objective 'expression.'[2] He differs from James also in that he refers to earlier writers who have, more or less explicitly, anticipated his theory: Spinoza, Malebranche and Bocalosi are quoted by chapter and verse, Lenhossék and Sibbern are mentioned.[3]

No one can doubt that James was thoroughly in earnest about his theory, or that he was eager in every legitimate way to recommend it to his readers. It is, therefore, the more surprising that he should not have added to his list of arguments the argument from the history of psychology; that he should not have followed up the clue offered him by Lange, and utilised his own wide reading for the marking down of other, more or less complete 'anticipations' of his doctrine. Even when he quotes Henle, with the remark: "Note how justly this expresses our theory!"—even then he fails to cite Henle's definition of emotion, occurring on the very same page, as "a presentation accompanied by sympathetically excited sensations, muscular movements, and secretions."[4] We must suppose that he had been "surfeited by too much reading of classic works on the subject" before the theory took shape in his mind;[5] and we must, perhaps, suppose further that, when the theory did take shape, it presented itself as something novel, as something opposed to the manner of the classical works; and that James consequently forgot the various hints that he had received from earlier writers. I return to this point later.

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[1] W. James, What Is an Emotion? *Mind* [O. S.], ix, 1884, 189; *The Principles of Psychology,* ii, 1890, 449. James quotes from the German translation of 1887.

[2] C. Lange, *Ueber Gemüthsbewegungen,* 1887, 3, 8.—I am not concerned, in this paper, to distinguish between the theories of James and Lange.

[3] *Ibid.,* 87, 88 ff., 92.

[4] *Op. cit.,* ii, 661; F. G. J. Henle, *Anthropologische Vorträge,* i, 1876, 72; *cf.* 64, 67.

Meantime I offer the following as a partial list of the 'anticipations' above mentioned. Several of them—Aristotle,[6] Descartes,[7] Spinoza,[8] Malebranche,[9] Lotze,[10] Maudsley,[11] Henle,[12]—I have quoted in my *Text-Book of Psychology.* I wish here to add to these bare references only two things: an appreciation of Descartes by Irons, and the translation of a paragraph from Lotze. "*Les passions de l'âme,*" says Irons, ". . . . will bear comparison with anything that has been produced in recent years. . . . The position maintained is similar to that now held by Professor James, but Descartes does not content himself with defending in a general way the assertion that emotion is caused by physical change. . . . The theory advanced is worked out with a completeness which is not to be found in the modern presentations of the same general point of view."[13] This is a strong statement; and Irons, though he was a careful writer and weighed his words, has not wholly escaped, in making it, the danger which besets all historical enquiry of this kind: the danger of a too explicit interpretation. Descartes is, in fact, neither so precise in thought nor so definite in statement as the reader of Irons' paper might be led to suppose.[14] Lotze, on the other hand, is as definite as one could wish. He has been speaking of certain moods of exaltation or excitement aroused by the hearing of music, the reading of poetry, and on a lower level by the movement of the dance; and he proceeds, in rough translation, as follows:

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[5] *Op. cit.,* ii, 448. It is significant that James here says: "Unfortunately there is little psychological writing about the emotions which is not merely descriptive." This statement seems to represent a generalised impression.

[6] See W. A. Hammond, *Aristotle's Psychology,* 1902, 6 ff., 211 f.; H. Siebeck, *Geschichte der Psychologie,* I, ii, 1884, 90; G. S. Brett, *A History of Psychology, Ancient and Patristic,* 1912, 138 f.; M. Dessoir, *Outlines of the History of Psychology,* 1912, 24. *Cf.* also G. L. Duprat, La psycho-physiologie des passions dans la philosophie ancienne, *Arch. f. Geschichte d. Philos.,* xviii. (N. F. xi.), 1905, 395 ff.

[7] R. Descartes, *Les passions de l'âme,* 1649, arts. 27, 29, 33, 36-38, 46, etc.

[8] B. de Spinoza, *Opera posthuma,* 1677: *Ethic,* trs. 1883 and later, pt. iii.

[9] N. Malebranche, *De la recherche de la verité,* 1674-5, trs. 1694, bk. v., ch. iii; *cf.* bk. ii, ch. iv.

[10] R. H. Lotze, *Medicinische Psychologie oder Physiologie der Seele,* 1852, 518.

[11] H. Maudsley, *The Physiology and Pathology of Mind,* 1867: *The Physiology of Mind,* 1876, 348 ff.; *The Pathology of Mind,* 1879, 222.

[12] *Op. cit.;* also *Handbuch der rationellen Pathologie,* 1846, 257; *Handbuch der allgemeinen Anatomie,* 1841, 754.

[13] D. Irons, Descartes and Modern Theories of Emotion, *Philos. Rev.,* iv, 1895, 291.

> "But even if the prime occasion of such a mood lay in the perception of intellectual harmonies, these sensory feelings, which are generated by the intellectual excitement, supervene upon its original content with a new and peculiar power to color the whole experience; while their arrest just as distinctly checks the development of the intellectual feeling. Our thoughts and desires differ, according as we are standing up or lying down; an unnatural and constrained position damps our courage; a comfortable sprawl is hardly compatible with devotion; all our anger evaporates if the body is unmoved; the hand which smoothes the wrinkles from the forehead brushes away the chagrin of which those wrinkles were the sign. It would be difficult to determine the limits of this influence, but there can be no doubt that its range is exceedingly wide; and it is a question whether our cooler judgment in the sphere of art and morals, or our reflective thought upon the danger or desirableness of some human state, does not depend for its life and intimacy upon this attendant train of sensory feelings, whereby that which is of value for its own sake is set for us in harmonious relation to the innermost conditions of our own individual existence. Our serene enjoyment of beautiful proportions is not simply this abstract pleasure; but in quickened heartbeat, in stronger and easier breathing, in the firm tension of the muscles, we feel our very self uplifted and sustained. Repentance and sorrow for the past are not simply a judgment of moral condemnation, inwardly pronounced and heard only by the soul; the flagging of our limbs, the lessened activity of breathing, the oppression of the breast,—in anger perhaps even the spasmic contractions of the bronchi, and the choking movement of the oesophagus that makes the food impossible to swallow,—show how the bodily organisation too tries symbolically to throw off a burden of contempt under whose weight it is groaning. Even the feeling of devotion is not a purely intellectual exaltation; with it, all unconsciously, our step takes on a less rapid pace, our movements become slower and more restrained, the body assumes a characteristic attitude, not of relaxation but of self-submissive strength; and all these bodily activities reflect into the soul's consciousness a feeling, which reinforces its intellectual mood."[15]

Style apart, these sentences might have been written by James himself,—though Lotze adds much that James would have rejected, and James ranks outright as constitutive the processes which, however great their importance, are yet for Lotze only contributory.

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[14] *Cf.* T. Böcker, *Die James-Lange'sche Gefühlstheorie in ihrer historischen Entwicklung,* 1911, 7 f.

[15] *Op. cit.,* 517 f. The paragraph occurs also in the art. Seele und Seelenleben, in R. Wagner's *Handwörterbuch d. Physiologie,* iii, 1, 1846, 198.

The first author to whom I now wish to call attention is La Mettrie, the earliest and most extreme of the French materialists. In the *Histoire naturelle de l'âme* (1745) La Mettrie outlines a vasomotor theory of emotion which was undoubtedly suggested by that of Malebranche, and which takes us a step nearer in time to the theory of Lange.[16] "Les nerfs qui tiennent les artères . . . paroissent donc, dans la colère et la joie, exciter la circulation du sang artériel, en animant le ressort des artères; dans la crainte et le chagrin . . . les artères resserrées, étranglées, ont peine à faire couler leur sang." In *L'homme machine* (1748) he reminds us rather of James.[17] "Que falloit-il à Canus Julius,[18] à Séneque, à Pétrone, pour changer leur intrépidité en pusillanimité ou en poltronnerie? Une obstruction dans la rate, dans le foie, un embarras dans la veine porte. Pourquoi? Parce que l'imagination se bouche avec les viscères: et de-là naissent tous ces singuliers phénomènes de l'affection hystérique et hypocondriaque." Here, too, we seem to have an echo of Malebranche.[19]

Next I refer to Cabanis, pantheist in philosophy and materialist in psychology, known in every text-book by a single quotation, but less read than his originality deserves. Cabanis may be introduced by a quotation from Lamarck (1809), who, after asserting that "moral feeling may, with time, exert upon the state of our bodily organisation an influence even greater than that which physical feeling is able to exercise," continues in this way:

> "Cabanis, noticing that persons who are habitually melancholy and depressed, oftentimes without any real cause, show a practically identical mode of change in the state of their [abdominal] viscera, drew the conclusion that we must ascribe the melancholy of these persons to this mode of change, and that these viscera contribute to the formation of thought. It seems to me that the learned author goes

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[16] J. O. de la Mettrie, *Oeuvres philosophiques,* i, 1796, 132 ff., esp. 137.

[17] *Op. cit.,* iii, 120.

[18] F. A. Lange (*Gesch. d. Materialismus,* i, 1908, 340; *Hist. of Materialism,* ii, 1892, 67) reads *Caius Julius.* But what should the great Caesar be doing in this galley? The reference is to that Julius Canus whose story is told by Seneca—and by Montaigne, who also narrates the deaths of Seneca and Petronius. See M. E. de Montaigne, *Essais* [1580], ii, 1862, 147; iii, 233; iv, 115.—Lange's mistake is repeated by G. C. Bussey, *Man a Machine,* 1912, 18, 91.

[19] Namely of bk. ii: so the context indicates. But La Mettrie may be using his own medical observations.

too far. No doubt the changed state of the organs, and especially of the abdominal viscera, often corresponds with the changes of the moral faculties, and even makes a positive contribution to those changes. But it does not at all follow, in my judgment, that this state contributes to the formation of thought; it helps simply to give the individual a leaning, which inclines him to find pleasure in one kind of thoughts rather than in another.

"Since moral feeling, if its influence is prolonged in some particular direction, definitely affects the state of the organs,—and there can be no doubt of this effect,—it appears to me that the recurrence of grounds for irritation, in the experience of a given individual, will have furnished the primary cause of the changes in his abdominal viscera; and that these changes, once they have been set up, will in their turn have established in this individual a leaning toward melancholy, even though there should at the time be no occasion for it.

"Parents may, it is true, transmit a disposition of the organs, that is to say, a state of the viscera fitted to give rise to this or that temperament or leaning, in a word, to this or that character; but it is further necessary that circumstances favor in the children the development of this disposition; otherwise they may acquire another temperament, other leanings, in a word, a different character. It is only in the lower animals, and especially in those which possess but little intelligence, that generation transmits, practically without variation, organisation, leanings, habits, all the various characters of the particular race."[20]

Lamarck gives no reference; he is writing from general recollection of the contents of the *Rapports du physique et du moral de l'homme* (1798-9—1802). The contribution to thought of the state of the viscera is discussed in Cabanis' first memoir;[21] the causes of habitual melancholy in the fourth, sixth and twelfth;[22] the possibility of transmission in the twelfth;[23] and the status of the lower animals in the second and tenth,[24]—though Lamarck may be thinking, in particular, of that striking paragraph in the sixth memoir, which reads like the exordium of a modern lecture on eugenics.[25] It is clear, at any rate, that he knows his author; and I have quoted him, partly to show how Cabanis' teaching was understood by a contemporary, and partly because Cabanis himself does not, so far as I am acquainted with him, furnish many quotable passages. His whole treatise seems to me to be informed with the spirit of the James-Lange theory; but I do not find it easy to offer a concise proof of this statement. Let us begin, however, with the doctrine of multiple centres.

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[20] J.-B. P. A. de M. de Lamarck, *Philosophie zoologique* [1809], ii, 1873, 267 ff.

[21] J. J. G. Cabanis, *Rapports du physique et du moral de l'homme,* i, 1824, 55. "Puisqu'ils [les viscères abdominaux] influent directement par leurs désordres sur ceux de la pensée, ils contribuent donc également, et leur concours est nécessaire, dans l'état naturel, à sa formation régulière."

[22] *Op. cit.*, i, 240, 242; ii, 121 (*cf.* the summary, ii, 170); iii, 304 f., 308, 320.

[23] *Op. cit.*, iii, 298.

[24] *Op. cit.*, i, 103 ff.; iii, 115 ff.

[25] *Op. cit.*, ii, 142. "Après nous être occupés si curieusement des moyens de rendre plus belles et meilleures les races des animaux ou des plantes utiles et agréables; après avoir remanié cent fois celle des chevaux et des chiens; après avoir transplanté, greffé, travaillé de toutes les manières, les fruits et les fleurs, combien n'est-il pas honteux de négliger totalement la race de l'homme! Comme si elle nous

> "There exist in the living body, independently of brain and spinal cord, different centres of sensibility, where impressions are, so to say, collected, like rays of light, either to be reflected straight toward the motor fibres or to be sent in a single bundle to the universal and common centre . . . We find three principal centres (exclusive of brain and spinal cord) . . namely: (1) the phrenic region, which includes the diaphragm and the stomach . . .; (2) the hypochondriac region, to which belong not only the liver and the spleen, but also all the superior abdominal plexus, a large part of the small intestine, and the great flexure of the colon . . .; and (3) a final secondary centre placed in the organs of generation, and including also the urinary system and that of the lower intestines. . . . The character of our ideas, the direction and even the nature of our emotions, enable us to diagnose the different physical circumstances [of these centres], while these same circumstances enable us to predict with certainty the moral effects which they will produce. . . . The nervous affections whose cause resides in the hypochondriac viscera . . . originate or develop all the emotions of depression and timidity." [26]

The phrenic and generative regions also play their part in the origination and development of emotions, but their operation is too complicated for brief quotation.

I give a few more examples of Cabanis' doctrine:

> "It is certain that a knowledge of human organisation, and of the modifications that temperament, age, sex, climate, disease may bring about in our physical disposition, throws a brilliant light upon the formation of our ideas; that without this knowledge it is impossible fully to understand how the instruments of thought operate to produce it, and how emotions and volitions develop." [27]
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> "The viscera which this [the abdominal] cavity contains, and especially the liver and the spleen, are liable to obstruction. Hence . . . those emotions which are slower to develop, but which are more profound and more incurable." [28]
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> "We see every day that acute or progressive inflammation of the brain, certain organic dispositions of the stomach, affections of the diaphragm and of the whole epigastric region, produce now an acute

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touchait de moins près! comme s'il était plus essentiel d'avoir des boeufs grands et forts que des hommes vigoureux et sains; des pêches bien odorantes, ou des tulipes bien tachetées, que des citoyens sages et bons! . . ."

[26] *Op. cit.*, ii, 154 f., 157, 162, 170; *cf.* i, 242; ii, 121; iii, 320.

[27] *Op. cit.*, i, 58; *cf.* Lamarck's passage, i, 55.

[28] *Op. cit.*, i, 240.

or transitory frenzy or delirium, now chronic mania or insanity. . . . It is not only the nature or order of ideas that changes in the various deliria; tastes, leanings, emotions, change at the same time. . . . Ideas and emotions take shape in virtue of the impressions received by the external organs of sense, and by the aid of those which are adequate to the most sensitive internal organs. It is proved by overt facts that these latter impressions may greatly modify all the operations of the brain."[29]

"Extraordinary irritations of the organs of generation may arouse in turn, according to the previous state of the system and their own degree of intensity, the disposition of the sanguine, of the bilious, and of the melancholic. . . . In general, to influence temperament, a disease must be able to assist in the production of constant dispositions of the organs."[30]

"In purely inflammatory phthisis, as soon as the slow fever is well established, the patient seems to experience a pleasurable agitation of the whole nervous system; he is full of cheerful ideas and chimerical hopes. . . . In phthisis caused by hypochondriac engorgement or by stomachic affection, the ideas entertained by the patient are, on the contrary, always gloomy and despondent."[31]

"In the animal system there may exist from the first, or may take shape as a consequence of habits of living, a larger or smaller number of those nervous centres which, although connected with and subordinated to the common centre, have their own mode of perception, exert their own kind of influence, and often remain isolated in their respective spheres whether as regards impressions received or as regards movements carried out."[32]

I can hardly hope that these extracts will prove convincing. Cabanis is apt to moralise when we should wish him to be concrete; he is interested in medicine rather than in psychology; and he is concerned with the larger problem of temperament, not with the transitory emotion. I repeat, nevertheless, that his work is permeated with the spirit of the James-Lange theory; its keynote is struck by Tracy in the sentence: "Aux différences et aux modifications des organes correspondent constamment des différences et des modifications dans les idées et les passions."[33]

Tracy himself has little to say of emotion, but that little is distinctly to our purpose.

"[Consider now] the impressions that we experience when we feel tired or fresh, dull or anxious, sad or gay. I know that you will be surprised at my placing states like these among the simple sensations,—especially the three last, which you will incline to regard rather as highly complex effects of the various ideas which are occupying the mind, and therefore as manifoldly compounded thoughts or feelings. And yet, just as we often feel tired and heavy without having gone through any considerable exertion, or experience a feeling of cheerfulness and well-being without having taken a prolonged rest, so it cannot be denied that we very often feel an unmotived anxiety, gaiety or depression. . . . The state of hilarity caused by a piece of good news and by a few glasses of wine, is it not one and the same? Is there any difference between the anxiety of fever and that of mental disquiet? Is it not easy to confuse the languor of indigestion with that of grief? Personally I know that I have often been unable to decide whether a depressed state of mind was due to depressing circumstances or to a disturbance of digestion. For that matter, even when these feelings are the result of our thoughts, they are none the less simple affections; . . . they are real products of pure sensibility; . . . in a word, they are true internal sensations. . . . The same thing holds of all the emotions, save that emotions properly so called always include a desire. . . . But the pleasant or painful state of the man who loves or hates a fellow man is a true internal sensation. *Je crois que tout ceci est entendu.*"[34]

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[29] *Op. cit.*, ii, 151-154; *cf.* iii, 304.

[30] *Op. cit.*, iii, 308 f.

[31] *Op. cit.*, iii, 57.

[32] *Op. cit.*, iii, 186 f.

[33] *Op. cit.*, i, Extrait raisonné fait par le sénateur Destutt-Tracy, xxxvii.—My reading of Cabanis is confirmed by the editor of the 1859 edition of Bichat's *Recherches* (see Note 35 below): "Bichat n'hésite pas . . . à placer les passions et les expressions sentimentales dans la domaine de la vie organique. Il ne fit toutefois qu'adopter à ce sujet la doctrine que Cabanis avait développée dans ses mémoires

In the year before Tracy's work appeared, Bichat—anatomist, physiologist, physician, "founder of scientific histology and pathological anatomy"—had already given to the world his epoch-making *Recherches physiologiques sur la vie et la mort* (1800). Here, fifty years before Lotze, is to be found an anticipation of the James-Lange theory every whit as definite as that which we have quoted from the *Medicinische Psychologie.* I cannot repeat everything that Bichat says; but I give a sample; and, for the rest, the book is easily accessible.

"Whatever relates to the understanding belongs to the animal life; . . . whatever relates to the passions belongs to the organic life. . . . The senses . . . act as simple conductors only, and have nothing in common with the affections, which they produce. . . . The brain is never affected by the passions; their seat is in the organs of the internal life. . . . It is astonishing . . . that the passions should have neither their end, nor beginning in the organs of this [animal] life, but on the contrary, that the parts which serve for the internal functions, should be constantly affected by them, and even occasion them according to the state in which they are found. Such notwithstanding is the result of the strictest observation.

". . . The effect of every kind of passion is at all times to produce some change in the organic life. [Consider anger, joy, terror, sorrow, and their bodily correlates.] The functions of the circulation, of digestion, respiration and secretion, are those which are most directly under the influence of the passions. . . . And not only do the pas-

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sur les *Rapports du physique et du moral*" (300). But Bichat's addiction to the James-Lange theory is beyond dispute.

[34] A. L. C. Destutt de Tracy, *Élémens d'idéologie: idéologie proprement dite* [1801], 1826, 26 ff.

sions essentially influence the organic functions, in affecting their respective viscera, but the state of these viscera, their lesions, the variation of their forces concur in a decided way to the production of the passions themselves. . . . In this way everything tends to prove that the organic life is the term, in which the passions end, and the centre from whence they originate. . . .

"The passions modify the actions of the animal life though seated in the organic life. . . . The most numerous sympathies exist between the internal viscera, and the brain or its different parts."

"There does not exist for the passions as there does for the sensations a fixed and constant centre; on the contrary the liver, the lungs, the spleen, the stomach, and the heart, are turn by turn affected." [35]

These sentences speak for themselves. It may be worth remarking, in connection with them, that Buffon, the famous author of the *Histoire naturelle*,—to which Bichat is heavily indebted,—makes the diaphragm the seat of the emotions. The claims of the brain are explicitly rejected; the diaphragm has the advantage both by its sensitiveness and by its position in regard to the internal organs of the body. An appeal is also taken to introspection:

"Pour peu qu'on s'examine, on s'apercevra aisément que toutes les affections intimes, les émotions vives, les épanouissements de plaisir, les saisissements, les douleurs, les nausées, les défaillances, toutes les impressions fortes des sensations devenues agréables ou désagréables, se font sentir au dedans du corps, à la region même du diaphragme." [36]

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[35] M. F. X. Bichat, *Physiological Researches on Life and Death*, trs. F. Gold, 1827, 54-74. I owe this reference to Professor J. W. Baird, of Clark University.—In a note to p. 57 F. Magendie takes the 'orthodox' view. "Does not everything lead us to believe that anger exists before the agitation of the heart, and that this is the effect of it and not the cause?" Bichat is also criticised by M. J. P. Flourens (*De la vie et de l'intelligence*, ii, 1858, 33 ff., 134 ff.), on the ground that it is necessary to distinguish between the seat of the passions and the parts of the body which they affect.

[36] G. L. L. de Buffon, *Oeuvres complètes*, xviii. (Histoire des mammifères, 3), 1824, 394, 396; *cf.* the account of Sympathy, 390 f. The volume was originally published between 1754 and 1767; I have not been able, with the bibliographical means at my disposal, to date it more exactly. Flourens sharply criticises Buffon's views of the brain (*op. cit.*, 141 ff.), but has failed to see (or at any rate to note: 32 ff.) this possible source of Bichat's theory. Readers of Buffon do not need to be told that he makes no effort to be consistent. It would be difficult to reconcile the passage here quoted with certain parts, *e. g.* of the *Discours sur la nature des animaux* (xvi, 1 ff.). The important thing in the present connection is simply that the passage was written.

Bussey (*op. cit.*, 189) quotes in another connection a sentence from Holbach, which wears the look of the James-Lange theory: "The passions are ways of being or modifications of the internal organs. ." Holbach says, however, that they are "states or modifications of the internal organ," *i. e.*, of the brain ([P. H. D. von Holbach] *Système*

La Mettrie had an immense influence upon later writers, but is hardly ever quoted; Bichat had a wide influence, and gets his acknowledgments. One of his followers, Dufour, takes up in 1833 a question which had greatly perplexed Magendie. "Bichat seems to say that the perceptions, which produce in us the passions, go directly and without the intervention of the brain, from the senses to the organs which he supposes to be affected by them. We cannot believe that such was his idea"; so writes Magendie, who proceeds to rationalise the whole process in his own way. Dufour, like Bichat, is a strong advocate of the James-Lange theory.

> "Our natural tendency to emotion does not take its source at all in our mind; it proceeds from the disposition of the visceral nervous system, which reports to the brain the excitation which it has thus received. . . . The difference [between emotions] springs from the special mode of vital function of our viscera or systems of abdominal organs, whose functions they in their turn enhance or abate. . . . The mind takes part in our emotions only subsequently to the nervous visceral disturbance which constitutes them; and the activity or inertia which they induce upon our organic functions exists before the mind takes cognisance of it."[37]

In a word, the brain is the centre of the intellectual, and the solar plexus the centre of the animal life.[38] But what, then, of Magendie's difficulty? Well, an impression (say, of sight) sends its excitation to the brain, and the brain's reaction "se fait sentir" at the same time upon the visceral system. The viscera react, in their turn and by way of pleasure-pain, upon the brain. What the mind takes cognisance of is this "double perception," of presented object and of emotive backthrow; and oftentimes the disturbance by presentation is so slight that the emotion comes without our knowing precisely what set it up.[39]

So far the viscera have had things their own way; they are the source of emotion, and their reaction upon the brain further influences the train of ideas and may determine action.[40] Now, however, Dufour turns to the counter-influence of brain or mind upon viscera or emotion. The finer or subtler emo-

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de la nature, etc., 1770, 115). There are passages of Holbach that anticipate Cabanis (121 ff., 154), but I find no anticipation of James and Lange: *cf.* 116, 146.

[37] P. Dufour, *Essai sur l'étude de l'homme, considéré sous le double point de vue, de la vie animale et de la vie intellectuelle,* ii, 1833, 388, 370, 389.

[38] *Op cit.,* 392.

[39] *Op. cit.,* 394.

[40] *Op. cit.,* 401 f.

tions actually derive from the mind, and the mind is able in any case to 'repress or favor' a nascent emotion.[41]

Blaud, a contemporary of Dufour's, offers a definition of emotion that is very like Henle's; an emotion consists of "affective perceptions or affective ideas, accompanied by a sensation."[42]

> "[We have in emotion] perceptions or ideas accompanied by a reaction of the brain upon some organ, more or less remote, and in general upon the epigastric viscera; a reaction which brings about in these viscera a perceptible modification, whence results secondarily a sensation of pain or of pleasure. . . . An emotion is the perception, pleasurable or painful, of an organic modification rapidly developed as the result of some impression made upon the senses, . . . or produced by some idea present to the mind or evoked by memory. . . . [There are] always [in emotion] these four constitutive elements: idea, reaction of the brain, modification of the vital organs, and pleasurable or painful sensation of this modification."[43]

We are again on the ground of the James-Lange theory. Blaud, however, is insistent that there is in man an "intelligent being," that man is constituted by a "spiritual principle";[44] and he therefore gives the primacy, in every case, to the mind or brain.[45] The "internal modification" which follows upon the conception of an idea "cannot be considered as cause of the movements of the mind; it is only their effect."[46] We may consequently have expression without emotion:

> "We can express, at will, all the emotions of which we are susceptible, without experiencing them. . . . That is the talent of the actor on the stage."[47]

And while expression may react on emotion, by way of reinforcement, it may also serve to externalise emotion and so to relieve it.[48] Finally, there is no bodily seat of emotion: if there were, the same emotion would have several different seats, and might even occupy several organs at the same time, "which is absurd."[49]

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[41] *Op. cit.*, 407 ff., 420 f., 427 ff., 446.

[42] P. Blaud, *Traité élémentaire de physiologie philosophique,* etc., i., 1830, 178.—I owe the reference to Blaud to a note in E. Boutroux, *William James,* 1912, 34. I have not seen the article by Nayrac there mentioned.

[43] *Op. cit.*, i, 322, 178; ii, 164. Blaud is fond of repeating these definitions: *cf.* i, 178 ("une ou plusieurs perceptions, une ou plusieurs idées, produisant, par l'intermédiaire du système nerveux, une modification organique perceptible, qui donne lieu à une sensation de douleur ou de plaisir"); ii, 164 ("une ou plusieurs idées avec affection organique perçue"); ii, 227 f.

[44] *Op. cit.*, i, viii f.

[45] Only in one passage do I find the suggestion of a limitation. "La perception de cette modification [organique], qui constitue seule le sentiment qui en est la suite, ne lui [to the brain] appartient point": i, 334.

[46] *Op. cit.*, i, 182 f.

[47] *Op. cit.*, i, 223. *Ct.* Plato, *Republic,* iii, 395.

Twenty years later, another French medical writer, Béraud (1853),—strongly influenced by Cabanis and Bichat,—ascribes the emotions to a reaction of the visceral organs upon the brain.

> "Simple changes in the state of the organs may bring it about that we are more or less disposed to emotions, to joy or depression or desire, by reason of the communication of the viscera, through the intermediation of the great sympathetic, with the part of the brain which presides over the instincts. An emotion like . . . anger, which would be impossible or infrequent if a man were fasting, becomes possible as a result of the modifications produced in the state of the brain by wine; the condition into which it throws the viscera being transmitted to the brain, it predisposes that organ to an emotion. . . . [So scientific men were brought] to believe that the seat of instinctive actions, of feelings and emotions, was in the various peripheral organs; they had not thought of the parts of the brain which the great sympathetic sets in relation to these viscera. . . . The affective life thus dominates and coördinates our entire existence, by interrelating the different parts of the brain and connecting these again with the internal viscera."[50]

The works of Dufour, Blaud and Béraud are, no doubt, of minor importance; but they indicate a trend; and I suspect that there are a good many other books of the same sort which discuss emotion, pro and con, in like terms. A view conceived in the spirit of the James-Lange theory was, in fact, traditional in French physiology. But there are signs of it in Germany as well. I refer, first, to Volkmann's *Lehrbuch der Psychologie* (1856); and I am glad of an occasion for mentioning it and its author. Volkmann's work can never be for us what it was for the psychologists of James' generation, text-book, source-book, bibliography, all in one; but it is still one of the great books in psychology; it is still indispensable if one would attain a clear psychological perspective.

After a descriptive account of emotion, in which full justice is done to the *somatische Resonanz* (*somatischer Nachklang, somatische Rückwirkung*),[51] Volkmann writes:

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[48] *Op. cit.*, i, 221 f.; ii, 303 (effect of clothes); ii, 216.

[49] *Op. cit.*, ii, 166 f.

[50] B. J. Béraud, *Éléments* [*Manuel*] *de physiologie de l'homme, etc.* [1853], ii, 1857, 623 f., 627. Béraud is in general concerned rather with the instincts than with the emotions.

[51] This was, of course, a feature of the Herbartian doctrine of emotion. See, *e.g.*, J. F. Herbart, *Sämmtliche Werke*, v, 1886, 77

> "In general, everything that excites or depresses the somatic life serves also to introduce, to intensify, to lengthen, the homologous emotions. Gout has always had a bad name for irritability. Chlorosis and a tendency to feverishness dispose to worry and anxiety. Hunger, cold, restraint of movement, dispirit and deject the mind. . . . Violent gesticulation, loud talk, violent walking to and fro, enhance and sustain anger: Kant advised that, if one would quiet the emotion of a visitor, one should above all induce him to take a seat. You may often get a gentle reminder of an emotion simply by imitating its bodily show; and the sculptors of antiquity were taught to put themselves in the posture of the particular emotion which they wished to represent. The story is told of A. Caracci that the painting of emotional situations always induced emotion in the artist himself; 'the hand which smoothes the wrinkles from the forehead,' says Lotze, 'brushes away the chagrin of which those wrinkles were the sign.'"[52]

And so he goes on to the counter-influence of emotion upon body.

I have next to quote a few sentences from another book of first-rate importance, Lange's *History of Materialism* (1866). They occur in the chapter on Scientific Psychology:

> "Descartes in his much-too-little regarded treatise on the emotions had already entered on the way of defining and explaining them by their corporeal symptoms. . . . In more recent times, Domrich in particular has the merit of treating thoroughly the corporeal phenomena by which psychical conditions are accompanied. but his work has been little used by the psychologists. It would necessarily be otherwise, if it were but generally seen in how high a degree the consciousness of our own emotions is only determined and brought about by the sensation of their corporeal reactions. Yet it is, in fact, with them just as it is with the consciousness of our bodily movements; an immediate knowledge of the impulse set up is indeed present, but we only attain to perfect clearness as to the phenomenon

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(Lehrbuch, § 106); x, 1891, 385 (Briefe über die Anwendung der Psych., 10); M. W. Drobisch, *Empir. Psychol.*, 1842, 208. T. Waitz goes farthest towards the James-Lange theory. In speaking of Sympathy he remarks: "Das Anschauen fremden Leidens, des physischen wie des psychischen, verursacht eine Nervenaffection von ganz eigenthümlicher Art. Es bleibt nicht bei dem blossen Vorstellungsbilde, sondern dieses verursacht einen organischen Reiz . .": *Lehrbuch der Psychologie als Naturwissenschaft,* 1849, 398; the whole passage, 396 ff., should be read in connection with 284 ff., 473 ff. ("die äusseren Zeichen an welchen sie [die Affecte] kenntlich werden und ihre Erklärung gehören in die Physiologie": 480). *Cf. Grundlegung der Psych.,* 1846, 201 f., etc.

[52] W. Volkmann Ritter von Volkmar, *Lehrbuch der Psychologie vom Standpunkte des Realismus und nach genetischer Methode,* ii, 1885, 389 ff., esp. 393; *cf.* 381 f., and T. Whittaker, *Mind* [O. S.], xv, 1890, 505, 508. A fourth edition of Volkmann's book appeared in 1894, so that it has kept its place in the sun for forty years,—no small achievement for a textbook!

through the backward rush of the sensations, which are occasioned by the movement."[53]

The Domrich here mentioned was a professor of medicine at Jena, who, in 1849, published a work entitled *Die psychischen Zustände, ihre organische Vermittelung, und ihre Wirkung in Erzeugung körperlicher Krankheiten.* The book, which seems to have been unknown to Darwin, contains a careful and detailed account of the 'expression' of emotions. Domrich gives the primacy to mind; he speaks of the "bodily changes caused by the emotions," of their "reaction upon the body";[54] but he is never tired of calling attention to the 'feelings' set up by these reactive influences, and of their enhancement of what James named the "general seizure of excitement."[55] Even in the cases of laughing and blushing he emphasises the 'feels' of tickling and of warmth; and the unmotived emotions are ascribed roundly to modifications of bodily organs.[56] I translate a few characteristic sentences:

> "These reflected bodily feelings have a far higher importance, for our whole psychical life, than is ordinarily believed. Many of the most attractive and most repulsive of our emotive traits (*Zustände unseres Gemüthes*) owe to them their origin, their continuance, and their characteristic coloring."
>
> "The activity of this or that region of the body is also increased, diminished, transformed. Such regions constitute the sounding board (*Resonanzboden*), which now echoes but faintly in sympathetic vibration, now is thrown into violent oscillatory motion which contributes essentially to enhance the emotion itself."
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> "We see . . . movement made onerous, the force of the muscles decreased, the feeling of self diminished, circulation, heartbeat and secretion modified; and, from all this, aggregate feelings of depression, oppression, heaviness, feelings of general despondency and of inner anxiety flow back to consciousness. . . These phenomena recur in all the really sorrowful emotions, and give them their peculiar cast."
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> "Many affections of the heart produce, without any moral reason for the emotion, such disturbance of mind and such uneasiness of

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[53] F. A. Lange, *Geschichte des Materialismus, etc.*, ii, 1908, 393; trs., iii, 1892, 183 f.

[54] O. Domrich, *op. cit.*, vi, 207 ff.

[55] *Psych. Rev.*, i, 1894, 523.

[56] *Op. cit.*, 239: "Dem Lachen geht gewöhnlich die Empfindung eines eigenthümlichen Allgemeingefühls vorher, welches man *Lachkitzel* nennt. Wir localisiren es in die Gegend des Zwerchfelles, woraus aber noch nicht folgt, dass es auch dort entstehe";—287: "In dem Augenblicke, wo das Schamgefühl entsteht, bemerkt man die Empfindung einer heissen Wärme, von welcher man kaum sagen kann, wo sie am stärksten ist. Wir fühlen sie im Gesicht, am Hals, in der Brust, hier leicht mit einem anderweitigen Gefühle verbunden." *Cf.* 15, 140, 164, 191 ff. (here Domrich quotes Lotze), 195, 220, 291, 296, 312 f., 322, etc.

conscience that the torture of the feelings thus occasioned is every bit as afflicting. There is, in fact, no feeling of anxiety without alteration of this bodily organ."[57]

Domrich, then, even if he is not a 'materialist' of the stripe of Bichat, nevertheless deserves a place among the partial anticipators of the James-Lange theory. He refers to Bichat, as maintaining that the source of the emotions is to be found in the organs of chest and abdomen; and with Bichat, in a matter-of-course way, he brackets Nasse.[58] This Nasse—a psychiatrist, and professor in the University of Bonn—was an indefatigable contributor to the medical journals of his time. In 1850 he published a volume entitled *Vermischte Schriften psychologischen und physiologischen Inhalts,* in which his principal papers were brought together. I have not been able to procure the book, nor have I had access to the article which Domrich evidently had in mind: the following quotations from other articles, however, may be taken as an indication of Nasse's position.

> "Since the abdominal organs and the heart exert, in the state of health, a considerable influence upon affective disposition, it may easily happen that, if their activity is in some way deranged, they give occasion, through their connection with the brain, to . . . irregularities of mental function."
>
> "Improved circulation of the blood and improvement in the state of the abdominal organs may change the affective disposition . . . into its opposite."
>
> "Affective disturbance proceeding from the heart is especially apt to induce despondency; that from the spleen is frequently connected with melancholy; a high degree of anxiety is often accompanied by pathological conditions of the intestine; suppression of the cutaneous activity sometimes, at any rate, brings with it an affective disposition of fearfulness and timidity."
>
> "The bodily functions that we may bring into causal relation with the origination and continuance of affective disorder are those . . . which in the state of health exercise a decisive influence upon the affective states. Heart, abdominal organs, skin, and in connection with all of these the blood, may be mentioned in particular."[59]

I have not been able to consult the works of Lenhossék and Sibbern.[60] I must therefore pass over these authors; and I conclude by translating the excerpt from Bocalosi. The first part of it (I have not seen the original) is not very clear:

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[57] *Op. cit.,* 131, 201, 299, 324; *cf.* 227, 228, 232, 235 f., etc.

[58] *Op. cit.,* 211.

[59] F. Nasse, Die Wichtigkeit der Beachtung von anhaltenden Gemüthsleiden zur Verhütung von Seelen-Krankheiten, *Rheinische Monatsschrift für praktische Aerzte,* i, 1847, 660, 661; Zur Unterscheidung der Gemüths-Krankheiten von anderen Krankheiten und unter sich, *ibid.,* 730; Die Gemüthskrankheiten, *Allgemeine Zeitschrift f. Psychiatrie,* etc., iv, 1847, 555.

[60] The works are: M. von Lenhossék, *Untersuchungen über Leidenschaften und Gemüthsaffecte, als Ursachen und Heilmittel der Krank-*

> "I call 'passion' of a body that sort of tendency and disposition which the component parts possess; and every body, regarded as a whole, has some kind of movement and action, depending upon an extrinsic cause; so that the particular action is the necessary effect of the particular cause."
>
> "If this is the true definition of 'passions' in general, we now obtain a clear idea of the word 'passion'; and it appears as a result that the nature, the diversity, and the greater or less energy of a man's passions must depend upon his particular organisation, upon the structure of his nerves, of his vessels, and upon the constitution and equilibrium of his humors. So that, to speak strictly, the passions are in the organs of a man, and not in his mind, and so their good and bad qualities must depend upon the make-up of the whole organic system, while the mind seems to be merely an attribute of this system or, if you prefer, is conditioned in its action upon the nature and constitution of the organ." [61]

We now return to the authors of the James-Lange theory. Lange, as we have seen, read the classical psychologists for anticipations of his theory, and found relevant passages in Spinoza and Malebranche. He missed the point of the *Passions de l'âme,*[62] and he appears not to have read as far down the centuries as Lotze. It is strange that he makes no reference to the French physiologists,—the more so, as he mentions Lenhossék; but we may suppose that he was definitely in search of his "vasomotor function," and thought it fruitless

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heiten, 1804; *Darstellung des menschlichen Gemüthes in seinen Beziehungen zum geistigen und leiblichen Leben,* 1824-5, 1834; and F. C. Sibbern, *Menneskets aandelige Natur og Vaesen,* etc., 1819-28, re-edited as *Psychologie,* etc., 1843, 1849, 1856, 1862; *Om Forholdet mellem Sjael og Legeme,* 1849. Lenhossék (1773-1840) was professor of physiology at Pesth and Vienna; Sibbern (1785-1859) was professor of philosophy at Copenhagen.

[61] Lange's citation is: G. Bocalosi, *Della fisionomia, principii derivati dall' anatomia, dalla fisiologia, e dinamica del corpo umano per mezzo de' quali si distinguono gli aristocratici, ed i realisti dai democratici,* 5th ed., Milan, year VI. of the republic (1797-8), 20 f. The passage is noteworthy, as showing what sort of statement Lange was disposed to consider anticipatory of his own view. It is worth remembering, in the same connection, that James unhesitatingly identifies his own and Lange's theories, whereas the resemblance is not mentioned, *e. g.,* in the editorial notice of Lange's book, *Mind* [O. S.], xiii, 1888, 304 f.—I have been unable to secure any bibliographical data in regard to Bocalosi, except that he was the author of works on *Democratic Education* and on *The Heart.*

[62] James (Note 5, above) finds in the classical psychologists "little which is not merely descriptive" of the emotions. Lange, oddly enough, finds in the definitions of the older psychologists little that is not "purely causal" (*op. cit.,* 82). So much depends on the attitude of the reader!

to examine scientific works of an age to which this "decisive physiological fact" was unknown.[63] The Bocalosi was, perhaps, the happy find of some half-hour in a library. Lange seems, however, to have composed his book in the orthodox scientific way; he made a conscientious effort to 'get up his literature,' as the phrase goes, before he proceeded to publication.

James' case is far more perplexing. We have remarked that James, in his chapter on The Emotions, mentions none of all the authors quoted in these pages, with the single exception of Henle; and the reference to Henle is, if I may so express it, just off the point of the James-Lange theory. Yet elsewhere in the *Principles* James refers to Malebranche, Lamarck, Tracy, Lotze, Maudsley, Volkmann, F. A. Lange,—refers, I mean, to those particular books from which I have been quoting. The finding of these names is, perhaps, not much more than an accident, since foot-notes and allusions give a very inadequate idea of the reading which precedes composition, and James has warned us that his bibliographies are "quite unsystematic"; but then it might be only chance, again, that has withheld the names of La Mettrie, Cabanis, Bichat. Could one, in fact, read the *History of Materialism* without becoming interested in La Mettrie? and would not that interest send one further to Cabanis?

The evidence must be indirect: but on one point, at any rate, I judge that James stopped short with F. A. Lange. "The phosphorus philosophers," he says (i., 102), "have often compared thought to a secretion. 'The brain secretes thought, as the kidneys secrete urine, or as the liver secretes bile,' are phrases which one sometimes hears." It will, I think, be agreed by all who know the *History of Materialism* that these sentences are an echo of Lange, who is speaking primarily of course of Moleschott and Vogt, but who also brings Cabanis into the group. Had James gone to the originals, he could hardly have written his critical paragraph as it now stands.[64] Cabanis, at all events, would have been perfectly content, in principle, with the admission that "the brain is the one immediate bodily condition of the mental operations," and that "the spiritualist and the associationist must both be 'cerebralists.'"[65]

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[63] *Op. cit.*, 87.

[64] Lange, *op. cit.*, ii, 1908, 134, 288; trs., ii, 1892, 242, 312. The devil is rarely so black as he is painted: and the comparison of thought, as secretion of the brain, with bile, the secretion of the liver, seems to have originated, not with the materialists, but with a critic of materialism! It occurs for the first time, so far as search has gone, in a letter addressed to Voltaire (1775), in which Frederick the Great is scornfully jesting at materialism: see G. Berthold, *Monatsberichte d. k. preuss. Akad, d. Wiss. zu Berlin, December* 1877, 765 ff.

If, then, James was satisfied in this instance with a secondary source, so may he have been also in other instances. There can be no doubt that he was temperamentally averse from materialism. He disapproved of anything in philosophy that was clear-cut, simple, dogmatic; but of the two simples and the two dogmatics he seems certainly to have preferred spiritualism. "Let not this view [of emotion] be called materialistic!" he exclaims in the *Principles,* and repeats the exclamation in the *Textbook*—where it reads a little curiously after the introductory statement that "at present Psychology is on the materialistic tack" and the formal adoption of that standpoint as the author's working hypothesis.[66] Why should the theory be called materialistic, any more than the view that "our entire feeling of spiritual activity . . . is really a feeling of

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The passage in Cabanis will be found *op. cit.,* i, 1824, 124 ff. It ends with the statement "que le cerveaux digère, en quelque sorte, les impressions; qu'il fait organiquement la sécrétion de la pensée." But the functions of the brain, in the same passage, are "de percevoir chaque impression particulière, d'y attacher des signes, de combiner les différentes impressions, de les comparer entre elles, d'en tirer des jugements et des déterminations,"—something very different from secretion! In fact, the analogy for Cabanis is simply that of functional specialisation, and by no means that of chemical manufacture. A fair translation of the final sentence would run: "We conclude . . . that the brain, so to put it, 'digests' its impressions; that it is an organ for the 'secretion' of thought."

C. Vogt's famous remark "dass alle jene Fähigkeiten, die wir unter dem Namen Seelenthätigkeiten begreifen, nur Functionen des Gehirns sind, oder, um mich hier einigermassen grob auszudrücken, dass die Gedanken etwa in demselben Verhältnisse zum Gehirne stehen, wie die Galle zu der Leber oder der Urin zu den Nieren" (*Physiologische Briefe für Gebildete aller Stände,* 1847, 206) is less carefully safeguarded; but here too it seems to be functional specialisation that is in point. *Cf. Köhlerglaube und Wissenschaft,* 1855, 33: "Den Beweis, den ich zur Widerlegung meiner Sätze verlangen kann: dass es eine vom Körper unabhängige Seele gebe; dass diese Seele nach dem Tode des Körpers fortleben könne; dass die Seelenthätigkeiten *nicht* lediglich Functionen des Gehirnes sind—diesen Beweis haben weder Herr R. Wagner noch Herr Lotze geliefert."—

I may perhaps be allowed to add here the references to the phrase "Ohne Phosphor kein Gedanke." See J. Moleschott, *Lehre der Nahrungsmittel, für das Volk,* 1850, 115 f.; *Der Kreislauf des Lebens,* 1852, 365 ff.; 1855, 376 ff.; 1857, 395 ff.

[65] *Principles,* i, 4. *Cf. Text-book,* 5 f.

[66] *Principles,* ii, 453; *Text-book,* 6 f., 380. The reference to materialism does not occur in the article of 1884.

bodily activities "?[67] And if it were materialistic, what harm, seeing that psychology is to be presented throughout from the 'positivistic' or 'natural science' or 'physiological' point of view? Only a temperamental aversion, brought to explicit speech by criticism, can explain the denial.[68]

There are, again, a number of passages in which James' reasoning shows a close correspondence with that of his predecessors. Since James would not have borrowed without acknowledgment, even where he felt to the earlier writer only the vaguest and most general indebtedness, it is clear that he was not consciously borrowing in these cases of definite argument. Consider, for example, the question that Magendie asks in reference to Bichat, and the reply that Dufour gives to it; and then note how James deals with the same problem in his article of 1884:

> "An object falls on a sense-organ and is apperceived by the appropriate cortical centre; or else the latter, excited in some other way, gives rise to an idea of the same object. Quick as a flash, the reflex currents pass down through their pre-ordained channels, alter the condition of muscle, skin and viscus; and these alterations, apperceived like the original object, in as many specific portions of the cortex, combine with it in consciousness and transform it from an object-simply-apprehended into an object-emotionally-felt." [69]

Could the parallel be closer? It is true that some such explanation as is here offered follows, naturally enough, from the main idea of the theory; and it is true that one swallow does not make a spring. But there are other parallels as well, some of them clearly indicated in the quotations that I have printed. James makes no references backward; he works out his arguments for himself; the one possible inference is that he was unaware of their earlier formulation.

But when these things have been said, there still remain the list of books that we know James to have read, and the immensely longer list that must lie behind. It is, to be sure, one thing to read a book through, and another thing to skim its pages in the quest of some special statement; and much of the scholar's reading is of the latter sort. Even so, however, the reader is not wholly constrained by his particular purpose; time and time again, when one is skimming a book in this way, it is positively annoying to be challenged by a remark, foreign to the interest of the moment, which insists on being noted down for later consideration. Since James was a highly trained reader, with manifold interests, he of all men would not have read blindly and narrowly. And, indeed, when I reflect upon his medical training, his personal relations with France, his wide acquaintance with French scientific writing, the general scope of his erudition, the indefatigable thoroughness with which he pursued a subject that he deemed worthy of attention, his high ideals of scientific procedure,—when I reflect upon all this, and place over against it the array of authorities cited in this paper, then the "fragmentary introspective observations" of the article in *Mind* leave me fairly puzzled; and when I think of his open-hearted recognition of Lange, I wonder all the more that he ignored Lange's references to the past. I have already suggested that, as the theory shot to a focus in James' thought, it carried with it a blaze of illumination; here, at long last, was something other than the classical descriptions and the endless classifications! But then, again, the mature scholar has learned by repeated experience not to take these illuminative moments at their face value,—has learned to doubt his originality and to question his inspirations. All in all, James' acceptance of the complete novelty of his theory must, I believe, be left to stand as something of a curiosity in the history of psychology.

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[67] *Principles,* i, 301 f. James here proposes to "dally with this hypothesis for a while"; nothing is said about materialism. In *Text-book,* 181, however, the hypothesis does not appear; and the introductory passage, 5 ff., despite the definiteness of its section-headings, is still distinctly apologetic in tone.

[68] If the criticism was printed, I have missed it; but the charge might have been made in conversation.

[69] *Op. cit.,* 203; *Principles,* ii, 473 f. (very slightly changed).

APPENDIX

The following list shows the principal authors from whom I have quoted. The books are arranged, so far as I am able to place them, in chronological order. Many of them, of course, went through several editions.

Malebranche (1638-1715), *De la recherche de la verité,*	1674-5
La Mettrie (1709-1751), *Histoire naturelle de l'âme,*	1745
L'homme machine,	1748
Buffon (1707-1788), *Histoire naturelle,* etc.,	between 1754 and 1767
Bocalosi, *Della fisionomia,* etc.,	before 1797
Cabanis (1757-1808), *Rapports du physique et du moral,* etc.,	1798-9—1802
Bichat (1771-1802), *Recherches physiologiques,* etc.,	1800
Tracy (1754-1836), *Élémens d'idéologie,* etc.,	1801
Lamarck (1744-1829), *Philosophie zoologique,*	1809
Blaud, *Physiologie philosophique,*	1830
Dufour, *Essai sur l'étude de l'homme,*	1833
Lotze (1817-1881), *Seele und Seelenleben,*	1846
Medicinische Psychologie, etc.,	1852

Nasse (1777-1851), ?	before 1849
Domrich, *Die psychischen Zustände*, etc.,	1849
Béraud, *Physiologie de l'homme*,	1853
Volkmann (1821-1877), *Lehrbuch der Psychologie*, etc.,	1856
Lange (1828-1875), *Geschichte des Materialismus*, etc.,	1866
Maudsley (1835), *Physiology and Pathology of Mind*,	1867
Henle (1809-1885), *Anthropologische Vorträge*,	1876

NOTE ON THE AFFECTIVE VALUES OF COLORS

By Samuel W. Fernberger, *Clark University*

While working over the data on the affective value of colors obtained from the laboratory drill class at Clark University in 1913, we found the results to be of such a nature, we believe, as to justify the following note. Fifteen subjects were used, all of them graduate students. The following twenty-one colors (Milton Bradley) were employed as stimuli:—red, orange, yellow, green, blue, violet and red violet, each of spectral saturation, tint 1 and shade 1. The colors were exposed upon a grey background and covered with a grey frame in which were cut two apertures 3.5 cm. square and separated by a grey strip 1.5 cm. wide. The method of paired comparison was employed, every color being shown with every other one. The subjects were instructed to give immediate judgments and reported no difficulty in doing so. Two separate series were used. In the first series, the subject was instructed to judge, in every case, which of the two colors was the more pleasant. In the second series, the subject was instructed to indicate, in the same manner, which of the two stimuli was the more unpleasant. The subjects were kept in ignorance of the results, as far as possible, until the completion of experimentation.

In Table I are to be found the frequencies with which each of our stimuli was judged pleasant and unpleasant. The values in this table are the average frequencies of judgment for all of our 15 subjects; that is, the absolute frequencies for each stimulus were added for all of our subjects and this sum was divided by 15. In the first column are found the colors of the stimuli. In the next column are found the average frequencies with which each stimulus was judged pleasant in the first series, in which the category of pleasantness was employed. The third column contains the average frequencies of unpleasant judgments upon our various stimuli from the second series where we employed the category of unpleasantness. The next column contains the values of the inverse frequencies of the unpleasant judgments found in the column just to the left. Finally in the last column are found the differences between the frequencies of the pleasant judgments and the inverse frequencies of the unpleasant judgments.

It will be noted that these differences in the last column are, on the whole, exceedingly small. On the average the difference is only 0.85. If we take regard of sign, the average difference is remarkably low, being only 0.081. The fact that pleasantness and unpleasantness are true psychological opposites is made very apparent when we plot the curves for the frequencies of the pleasant judgments and the inverse frequencies of the unpleasant judgments. These curves will be found in Plate I. The stimuli are laid off in order upon the abscissa and the frequencies are entered as ordinates. The two curves are very similar throughout their entire course and indeed, in most cases, are almost identical.

These results show, more exactly, we believe, than either the curves of Titchener[1] or those of Hayes[2] that pleasantness and unpleasantness are true psychological opposites. Or in other words, the process of judging does not seem to be influenced by the verbal form of the subject's announcement of the judgment. Furthermore, we believe, that this experiment recommends itself as an elementary drill procedure in the field of affection where introductory exercises are all too few. In most cases, the individual results of the several subjects were very nearly as remarkable as the group averages. A group average, such as the one given above, should certainly be instructive and of value to a class in an introductory course in psychology.

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[1] E. B. Titchener, Ein Versuch die Methode der paarweisen Vergleichung auf die verschiedenen Gefühlsrichtungen Anzuwenden. Wundt Festschrift. *Philos. Stud.*, XX, 1902, 382 ff.

[2] S. P. Hayes, A Study of the Affective Qualities. *Amer. Jour. of Psychol.*, XVII., 1906, 358 ff.

TABLE I.

Colors	Pleasant Judgments	Unpleasant Judgments	Inverse Frequencies of Unpleasant	Differences, Pleasant and Inverse Unpleasant
1 Red Tint[1]........	10.4	9.8	10.2	—0.2
2 Red.............	13.3	7.1	12.9	—0.4
3 Red Shade[1]......	10.9	6.9	13.1	2.2
4 Orange T[1]........	3.9	14.9	5.1	1.2
5 Orange..........	12.2	9.4	10.6	—1.6
6 Orange S[1]........	5.5	13.6	6.4	0.9
7 Yellow T[1]........	7.0	13.5	6.5	—0.5
8 Yellow..........	4.1	16.1	3.9	—0.2
9 Yellow S[1]........	8.8	13.5	6.5	—2.3
10 Green T[1].........	7.3	12.7	7.3	0.0
11 Green...........	11.3	9.6	10.4	—0.9
12 Green S[1].........	9.4	9.9	10.1	0.7
13 Blue T[1]..........	9.1	10.1	9.9	0.8
14 Blue............	14.5	6.6	13.4	—1.1
15 Blue S[1]..........	11.9	7.7	12.3	0.4
16 Violet T[1].........	10.3	8.3	11.7	1.4
17 Violet...........	13.7	6.3	13.7	0.0
18 Violet S[1].........	11.7	8.8	11.2	—0.5
19 Red Violet T[1]....	8.4	10.2	9.8	1.4
20 Red Violet.......	14.2	5.1	14.9	0.7
21 Red Violet S[1].....	11.7	8.2	11.8	0.1

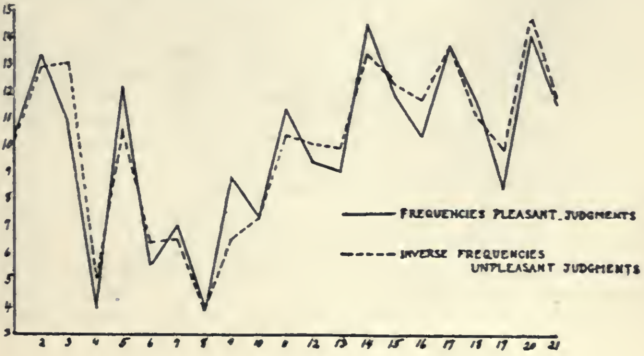

PLATE I

BOOK REVIEWS

The Judgment of Very Weak Sensory Stimuli with Special Reference to the Absolute Threshold of Sensation for Common Salt. By Warner Brown. University of California Publications in Psychology. Vol. I, No, 3, 1914. Pp. 199-268, pls. 1-4.

Although the term absolute threshold of sensation is found very frequently in the literature of psychophysics, Dr. Brown finds that the concrete experiences on which this notion is founded have received little consideration. In a former paper, the author made an analysis of the experiences that underlie the determination of the difference threshold. (The Judgment of Difference. Univ. of California Pub. I, No. I, 1910.) He feels, however, that the two cases are different inasmuch as at least one factor—the attitude of the subject-changes. In the case of the judgment of difference, the subject has two easy categories of greater or less which he can fall into, but in the judgment of the presence of a stimulus no such categories exist.

The author applied a salt stimulus and used two series of solutions; one containing degrees of salt from 0%-1.5% in steps of 0.1%; and the other varying from 0%-0.35% in steps of 0.025%. These stimuli were contained in bottles which were closed with a pipette, which latter would hold 1 cc., the amount always used for stimulation. The stimuli were presented in a chance order. Two groups of subjects were used. Each of eight subjects were given 40 series of the strong stimuli. These subjects were untrained in psychological procedure, they "were doing their first work in experimental psychology" (p. 210). The second group of subjects were four in number and these were more highly trained. On these the weak series of stimuli were applied in certain cases. In all, something over 10,000 judgments form the basis for the discussion.

Brown finds, as the result of certain series in which he recorded the order of presentation of the stimuli, that this order has an effect upon the judgments which will be given. This is conceived by the author as being due to subjective or "central" factors. "The central factors may be thought of as the condition of the organism itself modifying its reaction." Differences in reaction from time to time "may be thought of as due to the different condition of the organism" (p. 218). In the case of three observers, there was a tendency to reverse the preceding judgment. Brown also discovers the very interesting fact that the amount of difference between the stimuli themselves has an influence on the judgment. Thus the relative frequency of judgment on stimuli of the same intensity differed considerably when they occurred in the strong or the weak series. Hence the threshold was changed to a considerable extent.

But the main thesis of the paper is an interpretation of the threshold of sensation. Brown believes that the fact that a stimulus may be perceived in 50 or 75 *per cent.* of the cases depends upon too many irrelevant factors to make the determination of any value. In place of this concept of the threshold, the author suggests the following: When he plots the rough polygons of his "salt" judgments, he finds that with the lowest intensities, the curve at first rises slowly, and then more rapidly, and later more slowly with the stronger stimuli. Hence the steps in the middle of the polygon will be relatively higher than those at either end. "In such a graphic representation one step in the middle of the curve is higher than any of the others and marks the point where the curve changes from concave to convex." "Once it (this greatest step) has been discovered, it proves to be an admirable substitute for the conventional threshold." "This point can be defined precisely and has exactly the same significance for every person and for every kind of stimulus. It corresponds to the intensity of the stimulus from which a change of a given amount will be most frequently or easily recognized" (242 f). It is found furthermore, that this threshold falls very close to the old 0.5 threshold.

It would seem to the reviewer that Brown has not rid himself of the old notion that the threshold is an absolute value of almost mystical significance. The threshold has come to be recognized as a mere arbitrarily chosen probability, and as such it is open to variation due to all sorts of objective and subjective influences. All that can be claimed for a threshold value is, that it is the intensity of stimulus on which we may expect to obtain a certain judgment with a probability of 0.5 *under our given experimental conditions.* Brown has himself merely pointed out another of these conditions—the size of the steps between the stimuli. The factor of the effect of the order of presentation of the stimuli has been in the literature for a long time. Brown apparently fails to notice that the curves which he has plotted are merely the rough polygons from which we may construct the curve of the psychometric functions for the greater judgments. Such a curve has approximately the form which Brown describes, except for the one very significant fact that the central portion of this curve may be described as a straight line, or, in other words, there is no sharp change from concavity to convexity. Hence the localization of this point is even less definite than Brown would have us believe. A consideration of his own curves bears out this point. Inasmuch as we recognize that the threshold value is a mere probability, it would seem to be advisable to determine this as accurately as possible. The methods of calculation give the most probable value of this probability and Brown's method cannot attempt to even approximate this. Brown himself admits that the threhold, as ascertained by his method, lies very close to the 0.5 value. It would seem deplorable to substitute a less accurate measure for a more accurate one, when they give approximately the same value of intensity.

The reviewer does not believe that Brown's differentiation of the processes of judgment employed in obtaining the difference and stimulus threshold is valid,—not at least on the basis of his arguments. Brown states that in the judgment of difference the subject has the two easy categories of greater or less, while in the judgment of the mere presence of a stimulus no two categories exist. The reviewer believes that, for the obtaining of the stimulus threshold, there is very good introspective evidence that two such categories do exist—in the case of the present experiment these would be "salt" and "not salt." This of course may seem to be only a logical differentiation, but the reviewer believes that it is substantiated by introspective experience. Hence it would seem that there is no need of differentiating between the difference and stimulus thresholds.

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Zur Grundlegung der Tonpyschologie. By GÉZA RÉVÉSZ. Leipzig. Veit and Co., 1913. 148 pp.

The important feature of this book is an attempt to demonstrate experimentally the existence of a new qualitative attribute of pure tones. This second attribute the author calls "quality," as distinguished from the other qualitative attribute of pitch. The basis for the distinction is found in two kinds of "similarity": (1) that of tones lying near together (pitch similarity), and (2) that of tones which stand in the octave relation (octave similarity). While the latter has long been recognized in music, psychologists are not agreed whether it is due to an inherent attribute (Brentano), or to some extraneous factor such as partials (Helmholtz), or to fusion (Stumpf). Révész meets the argument of Helmholtz by employing pure tones, and offers experimental observations in reply to Stumpf.

His first task is to demonstrate octave similarity. This he accomplishes by a method of paired comparison; thus a standard series, such as $c\ c^1\ c,^2$ is compared with the series $g^1\ c^2\ f.^2$ The observers report that although the tones of the standard series are further apart, they are less "surprising" and more alike; later they report that the tones are "identical" or are repeated at different pitches. The observation is easy enough, although at first there is some difficulty in taking up the proper attitude of disregarding pitch similarity and confining the attention to octave similarity. The tonal scale, therefore, falls into a single linear series, and into a periodically recurring series; the first may be demonstrated by a quick *glissando* on the piano keyboard, the latter by a slow *glissando* with attention directed to the repetition of a tone.

Révész next proceeds to prove that the two kinds of similarity are based upon the two attributes of pitch and "quality." The attributes must be—and are—independently variable. Alteration of "quality" without alteration of pitch is seen in cases where observers are unable to judge whether a tone is higher or lower than another tone, and yet are able to report a difference of "quality." It frequently happens that unmusical observers are unable to report differences of pitch in intervals as large as thirds or fifths, although the difference of "quality" is clearly recognized. Changes of pitch without change of quality, on the other hand, are observed under normal conditions (1) in tones which stand in the octave relation, and (2) in tones below the lower limit and above the upper limit of the musical scale. As further evidence of the independent variability of the two attributes, the author offers results, previously published, which he obtained in the study of a case of defective hearing. For the defective observer, the "quality" of all tones over a certain range of the scale (g^2-*d-sharp*^{4}) was the same; *i. e.*, all tones in this area had the same quality as that of the tone *g-sharp*. Yet he was able, in 69% of the trials made, to judge the absolute pitch of all tones in the same area with no greater error than a semitone. Révész also believes that noise, in proportion as it is removed from tone, lacks "quality," while it always has a more or less definite pitch. Furthermore, the absence of the "qualitative" attribute in certain pathological or otherwise abnormal cases suffices to explain certain forms of melody-deafness.

Further evidence of the existence of the two attributes is found in cases of absolute pitch memory. Some observers are able to report immediately, with subjective certainty and objective correctness, the scale-name of the tone, but have difficulty in reporting the octave to which it belongs. Other observers have less difficulty in recognizing the approximate pitch of the tone, but show little subjective certainty and make frequent errors. The former type of observer employs the "qualitative" attribute as his criterion; the individual characteristic of the tone, its *c*-ness or *g*-ness, is recognized. The latter type of observer bases his judgment upon pitch. In most instances of absolute hearing, however, both criteria are employed.

Finally, the impression of intervals rests upon a combination of the two attributes. The separation of the tonal scale into two series which are independently variable is, after all, an abstraction; in actual experience we can no more separate the two attributes than we can separate the qualities of hue and tint in the colour series. It is for this reason that compound intervals (10ths, 12ths, etc.) do not sound like their corresponding simple intervals (3ds, 5ths, etc.),—as we should expect if the impression were based on "quality" alone. And it is for a similar reason that a normal interval (*c*——*e*) does not sound the same when inverted (*e*——*c*). There is, however, some ground for belief that within the octave "qualitative" difference obtains between inverted intervals,—a difference that results from the relative positions of the terms of the two intervals in the "qualitative" series; so that, if an individual were pitch-deaf from birth, he might still be able to differentiate inverted intervals.

The author's argument, as a whole, is impressive. Despite the fact that his conclusions are based partly on results obtained from a single case of defective hearing, and largely on experiments with the musical intervals of the tempered scale, one can hardly escape the conviction that he has succeeded in the demonstration of the two kinds of similarity, and has made out a strong case in favor of the dual attributive explanation. We regret his choice of the term "quality" as a name for the new attribute; some such term as "character" would have been just as descriptive, and the possibility of confusion would have been avoided. H. P. WELD.

Cornell University.

Backward and Feeble-minded Children. By E. B. HUEY. Baltimore, Warwick and York, 1912, pp. 213.

In this volume Huey has brought together some valuable material on the subject of mental defect. Recognizing the fact that the bulk of our feeble-minded population is in the high grade moron and border-line groups, and that the detection and care of these individuals is of the highest importance to society, Huey devotes the greater part of this book to detailed clinical pictures of thirty-two of these border-line cases. Each case was studied by the author personally during his residence in the Lincoln State School and Colony (Illinois) as head of the psychological department.

The studies of these cases show painstaking care and insight. Although Huey believes that the Binet scale is the best available collection of mental tests, and although he employs it as a measure of the psychological age of his subjects, he supplements it with other tests,—such as association reactions, orientation as to the points of the compass, correct placing of photographs of nine of the buildings of the institution on a rectangular table top representing the grounds, etc. Moreover, he establishes personal relations with his patient, plays games with him, and watches and interprets his reactions to as many situations as possible.

Several tables summarize the results of the physical and mental measurements and examinations; but while these have a certain statistical value, they are certainly less important and illuminating than are the individual case studies with their tentative prognoses and recommendations.

The author believes that the high-grade feeble-minded fall into some ten groups: viz., the dull; the unstable; the dull unstable (exhibiting now dullness, now flighty, excited reactions); the neurasthenically unstable (showing marked self-consciousness, dissatisfaction with their own performance, general irritation and discontent); the hysterically unstable; the epileptic; those showing "characteristic tendencies to insanity" ("marked incoherence of response and action, with little appreciation of the seriousness of their errors"); the morally unstable; those who show general mental deterioration due to meningitis, etc.; and finally the "relatively defective." This last group includes those with comparatively good mental endowment, who show mental weakness due to poor health, poor eyes, or poor environment. Huey maintains that neuroses are the "next higher rungs in the ladder of retardation."

A syllabus for clinical examinations of defectives is appended giving detailed requirements for (1) Home Record, (2) Teacher's or Attendant's Record, (3) Physical Examination, (4) Mental Examination. The first three are valuable, as representing a much more thoroughgoing investigation than is usually made. The last is merely a brief digest of Goddard's revision of the Binet scale.

The book ends with a statement of the mental functions which Huey believes should be tested. These have to do chiefly, in his opinion, with feelings and instincts, especially those centering around consciousness of self, with the "master function of sex always prominent."

The author believes that clinical psychology has yet to discover what mental functions are entailed in various forms of arrest, as well as to formulate tests of their efficiency. He tentatively offers the following list as comprising the most important of these: the function of completed action, in *rapport* with the environment; of attention; of synthesis,; of feeling; of learning, memory and ability to make report; of reasoning; of forming ideas and judging values; of self-direction; of normal associations; of fatigue and recuperation; and, most important of all, the function of maintaining a normal level of psychic tension.

Huey's classifications seem to the present reviewer to be of problematic value; his list of unsolved problems suggests an excellent program for future investigators. Undoubtedly the most valuable contribution made by the author is his careful and detailed description of the clinical findings obtained in the examination of his cases.

ELIZABETH L. WOODS.

Clark University.

Inductive versus Deductive Methods of Teaching: An Experimental Research. By W. H. Winch. Educational Psychology Monographs 11, Baltimore, Warwick and York, 1913. 146 p.

In this little volume Winch presents the results of an extended series of schoolroom experiments, directed toward a pedagogical problem of decided inport, by a man seasoned in the theory and practice of teaching. The author aimed to determine what are the relative merits of inductive and deductive methods of instruction; and he measured the advantages of the two methods by the amount of the newly acquired material which the pupils (8 to 15 years) could reproduce immediately and at subsequent periods; and also by the capacity which they showed for making a successful attack upon other material of an analogous sort.

The author's experimental construction of the inductive method consisted *1* in presenting homogeneous groups of drawings of geometrical figures—squares, or triangles, and the like—each group properly labelled and containing members of different sizes, orientations, and when possible, shapes; and *2* in obtaining original definitions, evolved by the pupils from these groups of sensory materials. By drawing upon the blackboard literal representations of the pupils' statements he led them to detect their mistakes, without giving them any positive information. His deductive method consisted in having the pupils learn simply worded definitions of these same geometrical figures, with concomitant reference to drawings of the figures themselves. Winch divided his subjects into two groups of approximately equal ability, upon the basis of preliminary tests in which the pupils were shown a group of figures belonging to a single class and were asked to define from these the group name, with or without the negative guidance mentioned. One group was then instructed in inductive fashion, while the other group learned the definitions deductively. The groups were then reunited and asked to define the names of the geometrical forms which they had learned. At intervals of a week, a month, or two months, Winch again obtained definitions of the same terms, thereby testing the pupils' retention of the material which they had learned by one method or another; and in the meantime—at intervals of two or three weeks after the learning—he presented properly labelled groups of unfamiliar geometrical figures of analogous sort, which the pupils then defined. In this way he obtained a clue as to the pupils' capacity for attacking successfully new material of a kind similar to the old.

The results show a constant, though relatively slight, advantage for the inductive method in all tests involving the capacity to define novel groups of analogous figures. In the immediate and the deferred reproduction tests, which showed high positive correlations, the results were not so uniform. In the case of two groups of older pupils the inductive method was the more successful, being followed by more complete recalls; while the deductive method was the more successful in three groups, two of younger, and one of older pupils. The last-mentioned group, however, contained large sub-groups of inferior pupils, among whom those who had been inductively taught were able to give more complete reproductions. Winch's pedagogical conclusions are, that the method of teaching which enables pupils to deal most successfully with new material analogous to that which they have learned should receive serious consideration; and that examinations, if they are to test good method in teaching, should always include questions on subject matter analogous to, but not identical with, that which is set down in the syllabus of instruction.

Winch's method is ingenious, and his work is extremely suggestive. From a purely psychological viewpoint, however, his analysis of the factors of induction and deduction is not sufficiently thoroughgoing. His manner of dividing his subjects is open to criticism on the ground that the method employed in his preliminary tests is practically identical with one of the methods of teaching which the experiment aims to evaluate, *i. e.,* his inductive method; and consequently in the division of the pupils, capacity for inductive defining is favored. This objection becomes more serious in view of the fact that his tables show relatively slight and sometimes insignificant differences between the results of his two methods. But the book should serve as a stimulus to every student who is interested in the experimental solution of disputed questions in educational science.

Clark University. S. C. FISHER.

Philosophie des Möglichen. Grundzüge einer Erkenntniskritik. Von Dr. Johannes Maria Verweyen. Leipzig, Verlag von S. Hirzel, 1913. Pp. vii, 240.

The nature of possibility is discussed by the author in various connections, such as contingency with reference to the existence or nonexistence of the universe as a whole, contingency in volitional conduct, and contingency in relation to physics, to historical method and to theology. With regard to contingency in general, he concludes that the most fundamental riddle is that of being *uberhaupt.* We are unable to grasp being in such a way that it is seen to include necessary existence. Contingency with reference to events within the existent universe is reduced to abstract or hypothetical modes of conception. As a consequence, indeterminism is ruled out of court, and it is shown that when disputes arise concerning the freedom of the will, the determinist argues from the standpoint of the really existent, whereas the indeterminist pleads for abstract variability, which means that with a different volition a different course of conduct will ensue. The question of conceivability is first distinguished from imaginability and is then decided by reference to a mind that possesses certain *a priori* potencies of thought, which is proved by the procedure of mathematics. It is conceded that the occurrence of miracles in the past can not be denied categorically, i. e., that a certain contingency of this kind in human affairs can not be proved to be impossible, but simply that the entire weight of historical evidence is against this supposition.

At various points in the presentation the reader has proper occasion to regret that the analysis is not pushed a step further. What, for example, is the nature of this stupendous riddle concerning being *an sich* or in the abstract? It is pertinent to inquire as to the conditions of any event within the universe, but when this inquiry is extended to the universe as a whole, are we becoming profound or merely foolish? If the wonder why something rather than nothing exists does not carry with it a reference to controlling conditions, what is its precise import? And similarly in the matter of miracles. if we do not assume an agency which does something, thus presupposing time, and which is yet non-temporal, what is the real point of the discussion? Problems of this kind solve themselves if we only succeed in giving a reasonably definite formulation of the point at issue. The nature of contingency is not to be decided by an appeal to abstract *Denkmöglichkeit,* but by a study of intelligence as a function in organic behavior. If we interpret intelligence in this way, many of our problems and their solutions become purely verbal. Moreover, the perennial question of the freedom of the will is then seen to be based on an assumption that is common to both parties to the dispute. An acceptable solution of the puzzle becomes impossible, since we are compelled to choose between a hypostatization of the will and pure mechanism, without attaining to a genuinely non-mechanical category to give freedom significance and value.

B. H. BODE.

University of Illinois.

Forgiveness and Suffering: A Study of Christian Belief. By D. WHITE, M. D. *Cambridge, University Press,* 1913. Pp. xi, 133.

The Flea. By H. RUSSELL. Cambridge, University Press; New York, G. P. Putnam's Sons, 1913. Pp. vii, 125.

A general account, in untechnical terms, which devotes some space to sense-organs, and discloses the secrets of the performing flea. The statement that "monkeys have no fleas" will be new to most readers.

Plato: Moral and Political Ideals. By A. M. ADAM. Cambridge, University Press; New York, G. P. Putnam's Sons, 1913. Pp. viii, 159.

A popular outline of Plato's ethical and political doctrine. The author, following the older tradition as against the views of Taylor and Burnet, believes that the greater range of thought in the later works reflects the development of Plato's own mental powers.

The Individual in the Animal Kingdom. By J. S. HUXLEY. Cambridge, University Press; New York, G. P. Putnam's Sons, 1912. Pp. xi, 167.

Defines an individual as a "continuing whole with interdependent parts"; and seeks to show that living matter always tends to group itself into "closed, independent systems with harmonious parts" which may be identified with the individuals treated of by the philosopher.

The Meaning of Evolution. By S. C. SCHMUCKER. New York, The Macmillan Co., 1913. Pp. iv, 298. Price, $1.50.

The First Principles of Evolution. By S. HERBERT. London, A. & C. Black; New York, The Macmillan Co., 1913. Pp. ix, 346. Price, $2.00.

These are popular books on the doctrine of organic evolution, both of them comprehensive in scope and philosophical in tendency. Professor Schmucker's volume is pleasantly written, but seems to belong to the past generation; Mendel's name, for instance, does not appear. Dr. Herbert's work, the outcome of a series of lectures to workingmen, is both fuller and more modern; it is lavishly illustrated, and will be found useful as a compendium of theories. The scientific reader will, perhaps, feel that there is rather too much of theory and will distrust the influence of Bergson; evolution, after all, so far as it is a scientific concept, must be worked out by observation and experiment. Books of this type, however, have a definite sphere of usefulness, and the two volumes before us are good specimens of their class.

Sadhana: The Realization of Life. By RADINDRANATH TAGORE. New York, The Macmillan Co., 1913. Pp. xi, 164. Price, $1.25.

This little volume is made up of eight essays, on the Relation of the Individual to the Universe, Soul Consciousness, the Problem of

Evil, the Problem of Self, Realization in Love, Realization in Action, the Realization of Beauty, the Realization of the Infinite. There is a distinct charm of style in all; but the thought is somewhat vague and desultory, with more than a touch of mysticism. While the book will make its appeal to readers of kindred temperament, it will not in general enhance its author's reputation.

Second Characters or the Language of Forms. By ANTHONY, Earl of Shaftesbury. Edited by B. Rand. Cambridge, University Press, 1914. Pp. xxviii, 182.

The Second Characters should have comprised four treatises: the Letter Concerning Design, A Notion of the Historical Draught of Hercules, An Appendix concerning the Emblem of Cebes, and Plastics or the Original Progress and Power of Designatory Art. The first two have been previously published; the third does not exist, and is here replaced by a translation of the Tablet of Cebes; the fourth is now printed for the first time. The definite grouping of the four treatises into a single work, as intended by the author, is also here first made known. The Editor has added an historical and analytical introduction, and has supplied translations and notes.

Phonetic Spelling: A Proposed Universal Alphabet for the Rendering of English, French, German and all Other Forms of Speech. By Sir HARRY JOHNSTON. *Cambridge, University Press,* 1913. Pp. vi, 92.

As the title shows, and as the author emphatically points out, this book has nothing to do with Simplified Spelling: "The proposals of the Simplified Spelling Society do not justify serious discussion" (p. 63). The writer, engaged in the preparation of a comparative grammar of an important family of African languages, has tried to devise an alphabet which shall serve, without too great refinement, for the recording of the speech-sounds not of the European languages alone, but of "every known form of human speech, including the Amerindian tongues and the click-studded Bushman language." No one can doubt that such a phonetic sign-system is needed; and no one who makes actual trial of the proposed symbols (based upon Lepsius' Standard Alphabet, though differing in many points of more or less importance) can doubt that Sir H. Johnston has achieved a solid measure of success. His alphabet must, of course, run the gauntlet of expert criticism, and must contend with the inertia of tradition; but it is a merit to have shown, in a way intelligible to the general reader, that the problem may be solved, at least approximately, without undue strain of eyes or memory.

The Soul of America: A Constructive Essay in the Sociology of Religion. By STANTON COIT. New York, The Macmillan Co., 1914. Pp. xi, 405. Price, $2.00.

An Introduction to Kant's Critical Philosophy. By G. T. WHITNEY and P. H. FOGEL. New York, The Macmillan Co., 1914. Pp. viii, 226. Price, $1.00.

A running analysis, largely in Kant's own words, of the *Critique of Pure Reason,* with connecting and interpretative observations. The book should be useful both for class-room work and as a *repetitorium.*

Historical Materialism and the Economics of Karl Marx. By BENEDETTO CROCE. Translated by C. M. Meredith, with an introduction by A. D. Lindsay. New York, The Macmillan Co., 1914. Pp. xxiii, 188. Price, $1.25.

"The Essays in this volume . . have all of them had an occasional origin. They bear evident traces of particular controversy. . . Their author thought it worth while to collect them in one volume . . . [as] an attempt to make clear by philosophical criticism the real purpose and value of Marx' work." The book has the interest that attaches to controversial writing, as well as a certain constructive value.

The Great Problems. By BERNARDINO VARISCO. Translated by R. C. LODGE. New York, The Macmillan Co., 1914. Pp. xi, 370. Price, $2.75.

A volume in Muirhead's *Library of Philosophy. I massimi problemi* appeared in 1910, and the present translation is made from the MS. of the second Italian edition. There are eight chapters (Search after Truth; Sense-perception; Memory, Feeling, Action; Cognition; Values; Reality and Reason; Being; Conclusion) and seven appendices (General Considerations: Theory of Knowledge; Limitations of Intelligence; Truth and Knowledge; Metaphysics and Morality; Thought and Reality; Immanence and Transcendence). The final result reached is that "things and facts are ultimately determinate forms of one self-identical Being, which coincides with our concept of Being." The chapters entitled Sense-perception, etc., are epistemological and not psychological; the appendices are for the most part controversial in character.

The Problem of Individuality: A Course of Four Lectures delivered Before the University of London in October, 1913. By HANS DRIESCH. London, Macmillan & Co.; New York, The Macmillan Co., 1914. Pp. ix, 84. Price, $1.00.

The first lecture deals with the general topics of mechanism and vitalism, and formulates the first argument for vitalism, that derived from the harmonious-equipotential system. The second lecture formulates the second and third arguments, those from the origin of the complex-equipotential systems and from human action; introduces the concept of the non-mechanical entelechy; and characterizes the entelechy negatively as non-energetic (the vital principle is not energy, does not create energy, does not in Cartesian fashion deflect energy), positively as exhibiting a suspensory or relaxing function. The third lecture takes up the logic of vitalism; distinguishes between singular, creative (thing-creative or movement-creative), and unifying causality; and decides that the concepts of wholeness and of unifying causality are fundamental. Even on Kantian lines a legitimation of the concept of wholeness is possible. The question of a suprapersonal entelechy, realising itself in space in the phylogenetic process, cannot be answered outright in the affirmative, by reason of the contingency apparent in every historical and political formation; but various facts, the moral consciousness in particular, attest it. The fourth and last lecture enquires whether vitalism may pass from dualism (singular *and* unifying causality, organic *and* inorganic Nature, contingency *and* order) to a monism of order. "Personally," says the author, "I confess that . . . I myself feel forced to accept the *dualistic* doctrine, in spite of all logical postulates." As regards the bearing of vitalism upon metaphysical questions, two points are noted: the Spinozistic dogma is contravened, since Nature cannot be entirely comprehended under spatial symbolism; and the postulation of entelechial predetermination may be taken either simply to mark the limits of our understanding, or to limit freedom of becoming: "the ultimate problem of the philosophy of wholeness is a matter of belief."

This is a rough account of the contents of the book,—which is useful, as summarising Driesch's conclusions in various fields of scientific and speculative activity, but which, just because it is such a summary, is far from easy reading. A minor feature that may be worth mentioning is the author's addition of foot-notes which bring his own ideas into relation with those of contemporary English thinkers.

BOOK NOTES

A HISTORY OF PHILOSOPHY. By FRANK THILLY. New York, Henry Holt & Co., 1914. 612 pp.

This book begins with the Greek philosophy of nature, which it distinguishes into the problem of substance, number, change, knowledge and conduct. Plato and Aristotle represent an age of reconstruction. Then comes the ethical movement, Epicureanism, Stoicism, Skepticism and Eclecticism; then the religious movement, Neo-Platonism and the Alexandrian philosophy; then the middle ages, Gnosticism, Apologisms, ending with Augustine; then Scholasticism proper, the decline and the Renaissance, and modern philosophy beginning with Bacon, Empiricism with Locke, rationalism with Leibnitz, the critical with Kant, German idealism with Fichte, then some twenty pages on German philosophy after Hegel, touching on Paulsen, Windelband, Rückert, Münsterberg, Dilthe, Endsen. Then comes philosophy in France and England, beginning with Comte, Saint-Simon, and touching on T. H. Green, Bradley, Royce, contemporary reactions against rationalism and idealism, and very brief mention of Mach, Avenarius, James, Dewey, Nietzsche, Bergson, etc. The book shows prolonged and diligent labor and the author has boldly ventured into the contemporary field, which is perhaps well, but it is no necessary disparagement of this part of the history to say that it will probably seem to every student, as it does to the writer of this note, that the selection of themes, not to say names, is somewhat arbitrary, and that in venturing into the theme of contemporary thought the author necessarily shows the limitations of a history in such a department as philosophy. Still, the book is one of great merit and we are inclined to think should supersede Weber as a textbook in American institutions.

Race orthodoxy in the south, and other aspects of the negro question. By THOMAS PEARCE BAILEY. New York, Neale Pub. Co., 1914. 386 pp.

Professor Bailey is a Southern man who, while connected with the University of Wisconsin, developed a unique type of school based on the nature and needs of children, which for a time promised to rival the famous school of John Dewey in Chicago. It unfortunately had to be abandoned. He has given years of study to the topic of this book, which first discusses the race problem and race prejudice, under which he treats Southern humanitarianism, curing the South, slave or surgery, fuse, fight or fail, the home and the habitation. Under reviews of typical views he discusses Murphy's basis of ascendancy, Southernism, "Uncle Tom's Cabin" sixty years after, Boas' "Mind of Primitive Man." Under views in a club he considers the organizations for the study of the negro question, the political view, the negro and the Episcopal Church. He then passes to race education, the rural negro school, the country institute, platform of principles and politics. Then comes the caste of the kin, under which title is discussed supposed racial traits, also racial sympathy and antagonism. Under freedom for the truth he treats experiences of a student of the negro problem and the need of a new problem. In the appendix he prints a number of brief papers on ancient Rome and the Southern race problem, the bearings of negro traits, Southern deterioration, syllabus of tentative scientific study on the negro and his problem, and statistics. As a whole the book is a very valuable contribution to the subject and should be a standard for all those interested in it.

The foundations of character; being a study of the tendencies of the emotions and sentiments. By ALEXANDER F. SHAND. London, Macmillan, 1914. 532 pp.

The writer first discusses the conception of character and a system of emotions, sentiments, particularly the sentiment of paternal love, and the sources of disinterested action; the theory that love and hate are emotions; are the sentiments commonly found among men; the analysis of hatred; the will as a constituent of character; the laws of mind and the difficulty of discovering them; the nature of the wisdom of life in fables, proverbs and maxims; the method of a science of character; the qualifications that a student should possess; the conception of conduct and its qualities; on the relative ethics of the sentiments; their relations to the virtues and ideals; sentiments as a source of character; influence of appetites; influence of natural tempers on the stability of sentiment. In the second book he treats of the tendencies of the primary emotions; relations of instinct; meaning of the term object-cause; varieties of fear; its common end; primitive varieties of anger; its end; the laws of the inter-action of fear and anger; the qualities that appear in them; the difference between joy and pleasure; play; sorrow; disgust; repugnance; surprise and curiosity. The third book discusses the system of desires which are related first to impulse and appetite, the laws of hope, despondency, anxiety, and despair.

Educational psychology. Vol. III, Mental work and fatigue and individual differences and their causes. By EDWARD L. THORNDIKE. New York, Teachers' College, Columbia University, 1914. 408 pp.

This third volume presents the physiological results of mental work and fatigue in part one, and individual differences and their causes in part two, while part three is a revision of the author's educational psychology which appeared in 1903 and later in 1910. Part one is entirely new. The same procedure of introducing topics by means of reports of typical investigations, presenting instructive evidence as well as conclusions and discussing the important principles of quantitative treatment in each case, which was adopted in the earlier volumes, is here maintained. Some of the most interesting chapters are those on mental work and fatigue, the decrease of efficiency of a single function under continuous exercise, the curve of work, the influence of continuous mental work, special or general, upon general ability, general theories of mental work and fatigue and their hygiene, measurement of individual differences, influence of sex, of remote and immediate ancestry or family, of maturity, of environment, nature and amount of individual differences in single traits, relations between amounts of different traits in the same individual, the nature and amount of individual differences in combinations of traits, types of intellect and character.

Interpretations and forecasts: a study of survivals and tendencies in contemporary society. By VICTOR BRANFORD. New York, Mitchell Kennerley, 1914. 424 pp.

The writer gives us first some illustrations of sociology such as the perfect citizen; the social, the socialistic and the sociological; social and sociological education; the citizen as a sociologist, the science of looking around and the art of creating Utopias. He then treats the citizen as psychologist, matriarchs old and new; the sociologist at the theatre, the morality play and its revival, from masque to opera, the eugenic theatre; the mediaeval citizen; the present as a transition, the people and their rulers, children and citizens at school, the university: youth and age in the cloister; town and gown in America; from social settlement to civic politics; eugenics and civics; the civic synthesis.

A son of the ages; the reincarnations and adventures of Scar, the link. A story of man from the beginning. By STANLEY WATERLOW. Garden City, N. Y., Doubleday, Page & Co., 1914. 334 pp.

The author of "Ab" here gives us an interesting and still more venturesome attempt to depict the development of man as man, and this he does in 14 stages,—the link, axeman, bowman, clansman, boatman, sowers, tamers, the deluge, kitchen middenites, lake-dwellers, armourers, sailors, the Hercynian forest, Alesia and the end. Each of these is a story based on considerable study of anthropology, describing a hero somewhat as if he were successively incarnated. The theme is a difficult one and treatment is interesting and suggestive. It is notable that it comes to us almost at the same time when Rutot has developed his statuettes illustrating the ten stages by which he thinks man has developed, from the pithecanthropus up to the historic stage.

Adventurings in the psychical. By H. ADDINGTON BRUCE. Boston, Little, Brown & Co., 1914. 318 pp.

This volume is somewhat in the nature of a sequel to the author's "Riddle of Personality," published six years ago, in which he reviewed psychological research in the realm of the abnormal with the purpose of making clear its bearings on the problem and the nature and possibilities of man. Here he undertakes a more typical and detailed treatment of the phenomena as scientifically investigated in the hope of reënforcing the new view of personality previously set forth, to "analyze the supernatural." The chapters are, ghosts and their meanings, why I believe in telepathy, clairvoyance and crystal gazing, automatic speaking and writing, *Poltergeists* and mediums, the subconscious, dissociation and disease, the singular case of B. C. A., the larger self.

Die Seele des Menschen. Von JOHANNES REHMKE. 4th rev. ed. Leipzig, B. G. Teubner, 1913. 109 pp.

This serviceable little book, in the same series as the above, here reaches its fourth edition. It first treats of the being of the soul, its determinations, the soul as changeable, man as a psycho-physical unity, the soul as a unit, as consciousness, objective, static and thinking consciousness, the soul as will, unity of consciousness; the second part, on psychic life, treats of the soul as objective, perceiving and conceptive consciousness, as personal thinking, and causal.

Psychology in daily life. By Carl Emil Seashore. New York, D. Appleton & Co., 1913. 226 pp.

Psychology is practical, and these chapters were originally semi-public addresses, some of which have been printed elsewhere. They are on play, serviceable memory, mental efficiency, mental health, mental law, law in illusion, and mental measurement. Few, if any, psychologists are more competent to deal with this subject, and we have not only here much old material well served up but much new.

Common sense; an analysis and interpretation. By Charles E. Hooper. London, Watts & Co., 1913. 172 pp.

This author first treats of common sense as commonly possessed, then as it seems in the light of discursive reason, then discusses mental traits and material objects, how the abstract is subordinated to the concrete, the universal to the singular. One chapter is on practical, including physical, common sense, another on social common sense, its relations to the philosophy of knowledge and of causation.

Education and the new utilitarianism and other educational addresses. By Alexander Darroch. New York, Longmans, Green, 1914. 169 pp.

The author treats the new utilitarianism, democracy, moral educational problems, the ideals of the end and woman's education, place of domestic science, the school and the state, is a science of education possible, the meaning and educational value of history.

Psycho-therapy; its doctrine and practice. By Elizabeth Severn. London, William Rider & Son, Limited, 1913. 211 pp.

This book treats of the science and art of healing, the psychological basis of mind, specific mental causes of disease, rationale of treatment, educational aspect of healing, and its spiritual significance, with an appendix on cases.

Nietzsche and other exponents of individualism. By Paul Carus. Chicago, Open Court Publishing Co., 1914. 150 pp.

This is an excellent little monograph with several new photographs of Nietzsche, some half dozen in all. It is written *con amore* and with an excellent knowledge of the now very voluminous literature. We are glad to note the increased activity of the Open Court Publishing Company.

Nietzsches Gefühlslehre. Von Hans Schaffganz. Leipzig, Felix Meiner, 1913. 133 pp.

"Nietzsche ohne Ende" we might well exclaim in view of the voluminous literature upon the subject, not only in Germany but elsewhere, some of the most important of which is given in the bibliography of this book. Like everything else about Nietzsche, his doctrines have to be divided into periods, and the stages of his views about feeling are three, the first represented by his birth of tragedy, and the second in his anti-metaphysical considerations, which stresses the distinction between fundamental and higher processes and the bearings of his feeling theory on his views of freedom, time, space, etc. The third period connects it most directly with his will to power and his theory of consciousness and its functions, and it is more vitally related to his views of morals and aesthetics.

Ernst Haeckel im Bilde; eine physiognomische Studie zu seinem 80. Geburtstage. Hrsg. von WALTHER HAECKEL, mit einem Geleitwort von Wilhelm Gölsche. Berlin, Georg Reimer, 1914.

After 17 pages of the sketch of his life and a eulogy, with an autograph poem to Friedrich Strauss, we have a series of 25 full page photographs and other cuts of Haeckel at different stages in life, beginning with the nineteenth year, where he is photographed with his parents.

Die Mechanik des Geisteslebens. Von MAX VERWORN. Leipzig, B. G. Teubner, 1914. 92 pp.

This interesting and very condensed little book treats of the body and the soul, processes in the elements of the nervous system, conscious processes, sleep and dreaming, suggestion and hypnotism.

Habit formation in a strain of albino rats of less than normal brain weight. By GARDNER CHENEY BASSET. (Behavior Monographs, Vol. 2, No. 4, 1914.) New York, Henry Holt & Co., 1914. 46 pp.

The heredity of savageness and wildness in rats. By ROBERT M. YERKES. (Reprinted from The Journal of Animal Behavior, July-August, 1913. Vol. 3, No. 4, Pp. 286-296.)

A graphic method of recording maze-reactions. By ROBERT M. YERKES and CHESTER E. KELLOGG. (Reprinted from The Journal of Animal Behavior, Jan.-Feb., 1914. Vol. 4, No. 1. Pp. 50-55.)

The Harvard laboratory of animal psychology and the Franklin field Station. By ROBERT M. YERKES.. (Reprinted from The Journal of Animal Behavior, May-June, 1914. Vol. 4, No. 3. Pp. 176-184.)

The study of human behavior. By ROBERT M. YERKES.. (Reprinted from Science, N. S. Vol. XXXIX, No. 1009. Pp. 625-633. May 1, 1914.)

Comparative psychology in relation to medicine. By ROBERT M. YERKES. (Reprinted from the Boston Medical and Surgical Journal. Vol. clxix, No. 22. Pp. 779-781. Nov. 27, 1913.)

The intelligence of earthworms. By ROBERT M. YERKES. (Reprinted from The Journal of Animal Behavior. Sept.-Oct., 1912. Vol. 2, No. 5. Pp. 332-352.)

Welt und Weltanschauung. (Reprinted from Die Religion in Geschichte und Gegenwart.) Tubingen, J. C. B. Mohr, 1914.

Un nuovo estesiometro. By AGOSTINO GEMELLI. (Reprinted from Atti della Società Italiana di Scienze Naturali. Vol. LII.) Pavia, Premiata Tipografia Successori Fratelli Fusi, 1913. Pp. 193-200.

Intorno alla influenza esercitata dalla posizione delle parti del corpo sull'apprezzamento di distanze tattili. By AGOSTINO GEMELLI. (Reprinted from Atti della Società Italiana di Scienze Naturali. Vol. LII.) Pavia, Premiata Tipografia Successori Fratelli Fusi, 1913. Pp. 275-305.

The Journal of Mental Science. April, 1914. Vol. LX, No. 249,. London, J. & A. Churchill. 350 pp.

Abnormal psychology. By WILLIAM BROWN. (Reprinted from the Sociological Review, January, 1914.) London, Sherratt & Hughes, 1914. 15 pp.

A time experiment in psychophysics; part II. By DARWIN OLIVER LYON and HENRY LANE ENO. (Reprinted from the Psychological Review. Vol. XXI, No. 1, January, 1914.) 22 pp.

Psychological factors in medical practice. By SHEPHERD IVORY FRANZ. (Reprint from Interstate Medical Journal. Vol. XXI, No. 1, 1914. 12 pp.

From Locke to Montessori; a critical account of the Montessori point of view. By WILLIAM BOYD. New York, Henry Holt & Co., 1914. 272 pp.

Freud's theory of the unconscious. By WILLIAM BROWN. (Reprinted from the British Journal of Psychology. Vol. VI, Parts 3 and 4, February, 1914.) Cambridge, University Press, 1914. Pp. 265-280.

Internationale Zeitschrift für ärztliche Psychoanalyse; hrsg. von SIGM. FREUB. H. Jahrgang, 1914, Heft 2, März. Leipzig, Hugo Heller & Cie., 1914.

Psychanalysis; its theories and practical application.. By A. A. BRILL. 2d ed. rev. Philadelphia, W. B. Saunders Co., 1914. 393 pp.

One hundreth annual report of the trustees of the Massachusetts General Hospital, including the General Hospital in Boston, the McLean Hospital and the Convalescent Hospital in Waverley, 1913. Cambridge, University Press, 1913. 333 pp.

Julius Robert Mayer; seine Krankheitsgeschichte und die Geschichte seiner Entdeckung. Von ERNST JENTSCH. Berlin, Julius Springer, 1914. 135 pp.

The negro races; a sociological study. By JEROME DOWN. New York, Neale Publishing Co., 1914. Vol. 2. 310 pp.

The mental capacity of the American negro. By MARION J. MAYO. (Archives of Psychology, No. 28, December, 1913.) New York, Science Press, 1913. 70 pp.

Experimental studies in judgment. By H. L. HOLLINGWORTH. (Archives of Psychology, No. 29, December, 1913.) New York, Science Press, 1913. 119 pp.

The rational education of the will; its therapeutic value. By PAUL EMILE LÉVY. Tr. from the ninth ed. by Florence K. Bright. Boston, Occult and Modern Thought Book Centre, n. d. 241 pp.

Education and psychology. By MICHAEL WEST. New York, Longmans, Green, 1914. 341 pp.

Studies in Bergson's philosophy. By ARTHUR MITCHELL. (Humanistic studies. Vol. 1, No. 2.) Bulletin of the University of Kansas, Vol. XV, No. 4, Jan. 1, 1914.

A CORRECTION

Dr. W. B. Cannon calls attention to the two following typographical errors in his article which appeared in the April *Journal:*

Page 272, line 3, read "orgasm" instead of "organism."

Page 277, line 10 of paragraph 2, read "almost without," instead of "almost with." [ED.]

CATTELL JUBILEE

On April 8th the former pupils of Professor James McKeen Cattell gathered at Columbia University to celebrate his completion of twenty-five years as Professor of Psychology. With very few exceptions all those who continued work in psychology so as to receive the degree of Doctor of Philosophy were present. At a dinner in the evening Professor Cattell was presented with a Festschrift in the form of reviews of his researches and of work in psychology to which these have led. The contents of this volume are as follows:

Professor Cattell's Work on Reaction Time.
V. A. C. Henmon.

Professor Cattell's Studies of Reading and Perception.
Walter F. Dearborn.

Professor Cattell's Relation to the Association Method.
F. Lyman Wells.

Professor Cattell's Psychophysical Contributions.
R. S. Woodworth.

Professor Cattell's Studies by the Method of Relative Position.
H. L. Hollingworth.

Professor Cattell's Relation to the Study of Individual Differences.
E. L. Thorndike.

COMMUNICATION.

WAUWATOSA, WIS., *April* 23, 1914.

My dear Doctor HALL:

Believing that an *amende* is due Dr. Adolf Meyer, Director of the Henry Phipps Psychiatric Clinic of Johns-Hopkins Hospital, Baltimore, on account of failure on my part to mention his services in a recent paper,[1] I take this occasion to state the circumstances to the *confrères* to whom the reprint of the paper was sent, members of the Am. Medico-Psychological and Am. Neurological Associations.

In the paper referred to I spoke of the "Ward for Nervous and Insane Patients," "Pavilion F," at the Albany Hospital, established in 1902, as one of the first steps toward a "Psychiatric Clinic" later to be evolved. I should have added that it was in the same year that Dr. Adolf Meyer, having been appointed Director of the Pathological Institute for the New York State Hospitals, inaugurated an advance of the greatest importance in the same direction. A friend who noticed the omission in my paper and kindly mentioned it to me, states that in his opinion Dr. Meyer's signal accomplishments at the hospitals on Ward's Island led to Mr. Phipps' munificent gift to Johns-Hopkins University and Hospital and to Dr. Meyer's own promotion to a field of higher activity.

My inadvertence resulted from the fact that my paper had reference mainly to the Middle West and was prepared under great pressure for time.

I make the above statement more on my own account than that of Dr. Meyer. The matter can be of little consequence to him, but I am unwilling to rest a moment even under the appearance of injustice to another.

I remarked in closing my paper that it was "cursory and imperfect." It was delivered at a semi-popular meeting attended by many of the laity where much technical statement would not be appreciated.

While making the above correction, I wish to supply one other omission in my paper. I did not include in my sketch an additional fact worthy of mention in connection with "Evolution in State Hospitals"; that is the development of the "Colony" as an outgrowth of public hospitals for the insane. This addition to such hospitals was first adequately described by Dr. Henry M. Hurd in 1887[2] and carried out with decided success at Kalamazoo, Michigan, and later at various other institutions.

Yours truly,

RICHARD DEWEY.

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[1] Forty Years' Evolution in Construction and Administration of State Hospitals in the Middle West. Reprinted from Illinois Medical Journal, issue of March, 1914.

[2] The Colony System as Proposed in Michigan. By Henry M. Hurd, Pontiac, Michigan, Proceedings Conference of Charities, Omaha, 1887.

Alexander Francis Chamberlain, Professor of Anthropology at Clark University, died April 8, 1914, after a short illness. He was born at Kenninghall, England, in 1865, but the family came to the United States while he was still a child, and after a short stay in New York State they moved to Peterborough, near Toronto, Canada. After a rapid course at school and collegiate institute, he entered Toronto University, where he received the B. A. degree in 1886 with honors in modern languages and ethnology. In 1887 he was appointed Fellow in University College, Toronto, which appointment he held for three years. During these years he made a study of the Mississaga Indians of Skugog, and he visited British Columbia for field-work among the Kootenay Indians.

In 1890 he was appointed Fellow in Anthropology at Clark University where he continued his studies under the direction of Professor Franz Boas, taking the Ph.D. degree in 1892, and presenting as his doctor's dissertation "The Language of the Mississaga Indians of Skugog." He entered the department as Lecturer in Anthropology in the fall of 1892, at a time when appointments in that field were rare in our universities and unknown in our colleges. He was promoted to the Acting Assistant Professorship in 1900, to the Assistant Professorship in 1904, and to the Professorship in 1911.

Chamberlain showed ability along literary and linguistic lines from early youth; and he was always interested in political affairs. He was a frequent contributor to periodical literature—a bibliography before us covering the years 1886-1910 contains no fewer than 711 titles. He rendered special service in the compilation of a yearly bibliography for the *American Anthropologist,* was for some years editor of the *Journal of American Folk-Lore,* and, since 1911, the active editor of the *Journal of Religious Psychology.* In 1896 he brought out his first book, "The Child and Childhood in Folk-Thought," which was followed in 1900 by "The Child: A Study in the Evolution of Man," which has gone through several editions. He contributed the article on the North American Indians to the eleventh edition of the Encyclopedia Britannica—an article that has attracted wide attention among ethnologists. At the time of his death he was at work on a Kootenay Dictionary, one part of which he had already completed, for the Bureau of Ethnology.

Dr. Chamberlain was deeply interested in subject races and was a staunch champion of the Indian and the negro. In politics he was a foe to all jobbery and "political pull," and he was known as an uncompromising democrat.

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THE DISTRIBUTION OF CONSCIOUSNESS AND ITS CRITERIA

By Robert MacDougall,
New York University

I

The field of psychology is by definition the system of phenomena which mental life presents. Its aim is to characterize these phenomena and formulate their laws under the ideal of a unitary system within which all descriptive and explanatory concepts shall finally be congruous with one another.

Materials for the psychologist's study are primarily afforded by the facts of his own immediate experience. But this field provides only the starting-point of his general inquiry, which at once carries him across the boundaries of his own mental life into a larger kingdom of minds. For this extension of the field there are at least two reasons. The first is methodological and results from the significance of the observer's attitude in psychological investigation. In order to study the mental content or activity in an unmodified form and under its natural conditions the mind must, so far as possible, be relieved of all other interests and responsibilities at the moment. Its participation in the conduct of the experiment,—such as the immediate control of conditions and presentation of the stimulus— and in the making and checking of records, should be reduced to a minimum. Among these modifying elements none is more significant than the attitude of the subject toward his own experience and its psychological significance; so that, in general, it is equally necessary to avoid an emotional self-consciousness and a reflectively critical attitude on the part of the observer. In many cases this can be secured only when the subject is in ignorance of the nature of the problem under investigation, and of the bearing of his own observation upon its solution. The conditions of experimentation have therefore made it the general practice of the psychologist to seek the assistance of an observer who is both freed from all care of what may be called instrumentation, and kept in an attitude of naïve or primary attention to the observation to be made. This first consideration, therefore, concerns only the difficulties of manipulation when the subject is called upon to control the conditions of his own observation.

The second reason for extending the materials of psychological study beyond the primary field of self-observation springs from the aim of the science itself. For its interest does not terminate with a scrutiny of the subject's own mind as individual and unique. That mind is indeed sufficient in itself to afford a field for psychological study, as various classical researches have shown; for it presents a complex system of correlated phenomena providing problems both of characterization and of explanation. But like all natural sciences psychology looks beyond the individual to series and types; and the object of the psychologist's study is a kingdom of minds to which his own belongs merely as one out of many. These minds are not identical in their forms and modes of activity. They vary individually and fall into groups according to their likenesses and differences. To explore these various mental types and to set forth their characteristic features, to formulate their variations and arrange them as typical groups in an orderly classification is the work of descriptive psychology. It is not personal characterization or individual history but the common features and laws normal to a group of individual minds in which interest centers.

In all this work the psychologist is indeed not brought face to face with his materials. He must reconstruct the form of any function, and of the mental life at large, by the interpretation of series of physical changes,—words, gestures, acts and modifications in material objects. The physical events are all that he directly perceives; the minds themselves lie beyond his field of vision and are discernible only indirectly. This dramatic reconstruction in terms of a significant inner experience we make, of course, quite apart from any scientific interest; and psychology may be said simply to adopt the interpretative reconstruction which common sense has made on the basis of the purposeful system of activities connected with the human body. It is a general predication of the relation which immediate experience presents. As my own bodily activities express my mental life in the sense of presenting specific correlations with its individual features, I interpret each individual movement of my neighbor's body in terms of a similar mental change.

Psychology thus avoids the metaphysical problem which a criticism of this process of interpretation raises. Mental life is postulated. The problem of real existence and its cognition is not raised. Yet the product of common-sense interpretation cannot be accepted uncritically as the basis of a psychological predication of the existence and range of consciousness. The procedure of common sense is loose; it applies no rigid criterion; it is not consistent in its successive attitudes. The range of significant inner life is now broadened and then restricted as purpose and sentiment change. In the course of these shiftings conflicting criteria are applied at different times, the same process being interpreted now in terms of purpose then in terms of mechanism. At one moment the whole order of nature is made the expression of a significant spirit, at another this form of interpretation is denied even to the subject's own acts.

Psychology cannot accept a principle so loosely applied as the basis of its treatment. Either nature is ensouled and every change occurring therein is expressive of an appreciative inner life, or it is not so. In the first case we are theoretically confronted with the task of formulating the psychology not only of men and brutes but of unicellular animals and plants, not only of living organisms but of inanimate objects and the earth as a whole; for under the assumption in question all these existences are part of that general field with which psychology deals, whether it be accessible to the application of technical methods or not. In our sentimental moods we do thus apply to all changes of nature the concept of a significant life, a life of function and purpose, of sensitivity and appreciation.

The problem, after all, cannot be wholly avoided. In those types whose affiliation with our own is evident and close the problem may not be raised, except in terms of the particular trait of which the physical datum is to be taken as evidence. But when the divergence has increased to a certain extent, when both habit and structure are materially different from our own, it is certain to arise. What limits shall we set to the distribution of mind? Which class of facts shall we accept as the indication of its presence?

To formulate this problem rationally we must approach it with an understanding of the criteria according to which mind is predicated in connection with other physical activities than our own bodies. The argument has been called an analogy; it may also perhaps be called an induction. The alliance of physical and mental we know directly in the case of our own minds and bodies. Each side here is immediately given. The parallelism is not indirectly verified but immediately intuited. Every change in the body is sensed; every intention, every feeling finds expression in a characteristic bodily modification. Nor is the connection a merely general association; it is a point by point correspondence which may be read from either side. Thus, if my meaning is expressed in words, this relation necessitates that my words should have meaning,—that is, the reproduction of the words in their proper order should be capable of reinstating the thought they originally expressed. One thus becomes able to read meaning into words as well as to express meaning through words. And so also with every other factor in this physical correlate of mind; each expression is a symbol for a certain mental content which may indirectly be reached through it.

Now in the physical world there is to be found alongside of the observer's body other similar bodies reproducing his own feature by feature and manifesting the same general system of functions. The body with its activities, in these other cases, is looked at from without. Its form and functions correspond in general with the observer's own. Concerning the question at issue regarding this physical organism the only ground for inference is immediate experience. Within that field falls one case in which such a system of activities is known to be more than physical change, namely, in the case of the observer's own body. This, however, is not one among a number which do not conform to its type; it is not an isolated case in a larger field, an exception to the rule. On the contrary, it is the sole case known to him, and the situation may be stated thus: Wherever consciousness is known to exist it is in correlation with a physical body and its changes. The one instance exhausts the universe of experience. If, then, in the only case in which the problem of physical and mental correlation is actually resolvable, the connection be found to exist it is logically to be predicated in all those other cases in which one of the two correlated factors is given. An appreciative inner life is therefore asserted in connection with the activities of all human bodies alike.

The system of physical activities which constitutes the basis of inference in the case of other persons cannot of course be observed with any approach to the intimacy with which it is known in the case on our own bodies. Little of that complex manifold of outer and inner changes of which we are immediately aware is apparent to another observer. One cannot hope, then, by the inspection of such aspects as are discernible in another body to arrive at an adequate acquaintance with the correlated mental attitudes. Nevertheless the likeness is sufficient to warrant us in the general ascription of a mind which duplicates the main features of our own while departing from its type in a variety of particulars.

There is perhaps no one element of these bodily changes which we can call essential to the correlation in question,—such a physical activity that its absence would entail a denial of the mental correlate in the given case. To differ from our own body in specific points, we say, is not to lack a mind in its entirety but to possess a mind differing from our own in individual features. We make provision for a series of divergent types within the realm of minds.

Since the basis of interpretation in all cases is a system of physical changes, and the associated type of mind is conceived to be reflected in its characteristic forms, the mental system will vary correlatively with alterations in bodily structure and function. Now, in the case of the physical correlate there is presented a series of progressively diverging types which, without essential break, carries us forward to forms extremely unlike our own,—organisms in which every individual feature is altered, which present none of those special structures or activities upon which the interpretation of mental constitution in our fellow-men depends. Indeed the series may be carried continuously through the succession of organic types until we have lost all but the most general properties of living substance and are confronted,—for example in the world of plants—with forms and relations which afford almost no practicable ground of comparison with our own.

If the imputation of mental life, in either its specific or its generic characters, be dependent upon specialized activities or connected with forms of structure, that life manifestly cannot be attributed throughout this whole series of forms. We must formulate the special criteria upon which our attribution turns and seek to define the limits within which alone we apply this mode of interpretation. In considering the distribution of mind, however, there are two questions to be taken into account: the first touches our criterion of the possession of mental life at large; the second affects the evidence for the existence of a conscious aspect in the case of any individual physical activity.

II

The problem of the extent of correlation between physical and mental changes is not met for the first time when the limits of human activity have been passed, nor even when the interpretation of expressive reactions on the part of other men is undertaken. The observer is confronted by it within the field of his own immediate activity, in the problem whether all bodily processes are accompanied by consciousness, and *per contra,* whether all these processes are subject to modification by consciousness. The latter question need not here be considered. The remaining problem presents three possibilities:—first, that the whole system of physiological changes is reflected in consciousness; second, that there are certain activities which as a class are always unconscious and others which likewise are characteristically accompanied by consciousness; and finally, that any individual process may now have a form of consciousness correlated with it and then lack such an accompaniment, according to the conditions under which it occurs.

The classical answer to this problem is that only one form of bodily activity is immediately connected with mind, namely that of the central nervous system; and that any other physical or physiological process must be mediated by nervous activity if it is to be represented in consciousness. But as the central nervous system is both geographically limited and protected against direct exposure to stimulation arising in the external world it does not, without supplementation, provide the basis for a representation either of the external world or of the organic changes occurring in the body at large; nor is it fitted to be a medium for the expression of mind either in terms of characteristic changes within the body which parallel the mental attitude, or of transformations wrought in the external world through its agency.

On the other hand, the nervous system is made co-extensive with the body by an intricate system of outgrowths which penetrate every part of the organism; and it is brought into relation with the external world through the system of end-organs attached to it. In its constitution also the nervous system represents the two relations in which the connections of mind and body are characteristically read: it comprises a sensory aspect which makes possible the representation of the series of special stimulations acting upon the end-organs of sense in terms of central nervous change; and it comprises a motor aspect which makes possible the expression of the series of central changes in terms of muscular contraction. In this sense the central nervous mass has been called a projection system for the whole series of sensitive and mobile points comprised within the limits of the organism.

The nervous system thus stands in unique relations with the body as a whole. It has no such specific function as have stomach, lungs, heart and kidneys. It contributes no new physical characteristic to the body. It does not even form the necessary condition of discrimination and adaptive response. Its function is to be called general rather than specific. It co-ordinates and integrates the whole system of bodily activities; no organ or living tissue but is penetrated by its fibrils, no function or reaction which proceeds in independence of it. Touching every part of the organic system and reflecting both its sensitive and reactive aspects the nervous system is thus aptly constituted to function as the physical basis of consciousness, if we view the latter in terms of its correlation with the whole sysem of activities which the body manifests.

In order that consciousness shall be predicated under this conception there must finally be an activity within the nervous system, and such activity will be accompanied by a characteristic consciousness whatever its original cause. No other physical process, on the other hand, will ever be accompanied by consciousness unless it finally gives rise to some form of central nervous change. But as every part and function of the body is connected with the nervous system the necessary conditions are given for the reflection in consciousness of the state of activity in the body at large and of each specific change occurring within it. We cannot restrict the system of modifications thus represented to limits narrower than the total group of functions with whose structural basis the elements of the nervous system are interwoven, and must therefore say: the activities of the body as a whole are representable in consciousness, in the sense that a change occurring in any part of the body may produce a definite effect upon the consciousness of the moment.

On the other hand, if the possibility of an arrest of functional activity within the central nervous system be admitted, or an interruption of continuity between that system and the rest of the body, there will be no necessary representation in consciousness of any process occurring without its own immediate bounds. Whether, in that case, any individual change is to be represented in consciousness or not will depend upon the functional integrity of its locus with the central nervous system at the moment. As conditions change it will now be represented and then not.

The whole systematic correspondence between mind and body will thus be thrown into doubt, since their relation depends upon the permeability of lines of connection between the central nervous system and the rest of the body. If these lines be closed, neither will the peripheral change affect the central nor will the central modify the peripheral,—neither will stimulation be reproduced in consciousness nor will mental attitudes find expression through muscular change.

In the interpretation of physical processes in terms of mind a question of validity is thus raised at the outset; it cannot be deferred until outlying types of organic life are reached. Even in our own bodies the situation is obscure, and it has only been at the close of a long and slow historical development that technical students have themselves adopted as their working hypothesis the concept of uniform correlation between mental and physical changes even in the relation of the central nervous system to the mind. In every case it is indeed rather the logic of the situation than specific evidence which determines our judgment in these matters. It is impossible to indicate the minute ramifications of that process of bodily change in which our mental attitudes find expression, and it is equally impossible to trace in consciousness the reflection of each element of that vast and subtle system of modifications occurring in the body as the result of physiological conditions and physical stimuli.

The fact that any individual change in the body may or may not be represented in the consciousness of the moment, according to circumstances, enormously complicates the situation by which the psychologist is confronted when he inquires concerning the distribution of mind; and out of it arises a series of special problems. The general question of representation in consciousness may be raised concerning any bodily activity whatsoever since modifications in the central nervous system are hidden from psychological observation, the data upon which interpretation proceeds being invariably peripheral in character. Nor is the range of ambiguity limited to the series of sense-impressions in their relation to central nervous activity. By a further complication the products of motor nervous activity themselves are subjected to doubt. These processes, which by assumption are the direct result of central nervous activity and are habitually accepted as the characteristic expression of mental attitudes, do not necessarily occur in isolation from the activity of the central nervous system when their significance is thus questioned. Within that system itself a division between higher and lower is found necessary, the higher levels of nervous change alone,—those namely which depend upon the cerebral cortex itself—being conceived as the essential correlate of mental activity. If the motor reaction does not involve these higher centers it will then have no representation in consciousness.

The division of peripheral activities to which this conception gives rise does not present two separate groups depending upon different muscular tissues, the striped and unstriped for example. It is the same movement which is now conceived to involve cortical activity and then to proceed in independence of it. In this fact lies the peculiar difficulty of interpreting reactions in terms of their mental correlates. The physical datum bears all the characteristic marks of a consciously determined act. It is an adaptive response to the situation and objectively expresses intelligence and purpose. It may both present a high degree of complexity in itself and represent an external situation which is similarly complicated. Its form may be such as could result only from a long-continued process of training originally dependent upon high intelligence. To conceive such a reaction as occurring independently of consciousness at any stage of its history may thus be a matter of difficulty, and its proper interpretation is rendered still more complicated by this persistent tendency on the part of the observer.

The psychological significance of any act, as well as of any impression on a sense-organ, will thus depend upon its relation to the central nervous system, not in the sense of its general structural or functional connections but of the situation which exists at the moment between the peripheral process in question and functional activity in the highest parts of that system.

The evidence upon which judgment turns in such cases will be sought either in a determination of the relations in which the two processes stand, according to physiological or histological methods, or it will take the form of an objective interpretation depending upon the whole logical situation presented. The first of these alternatives is completely impracticable as an experimental means of settling the individual case, and its inferential application is excluded by our lack of knowledge concerning the actual neural conditions under which such continuity is or is not to be expected.

Thus in a series of special activities lying within the field of normal adult experience the question of psychophysical correlation is already raised. Which process is reflected in consciousness, which is dependent upon consciousness, cannot be told on the basis of specific character in the physical activity involved. Both stimulus and reaction share in this ambiguous character. The impression on eye or ear may have no representation in consciousness if functional integrity with the central nervous mechanism be at the moment interrupted. Such breaks appear to be caused incessantly by the distribution of attention. Familiar impressions,—continuous or repeated sounds, contact with clothing, odors in the room in which one is at work—thus fail of representation. In sleep the connection is so far broken that the general system of sensory impressions is shut off from the cerebrum. Yet we know that these stimuli from the outer world may occasion changes in the dream content and thus be reflected in the sleeper's consciousness.

The question whether such connection may not be more widespread than is supposed throws this whole series of relations into doubt. The distribution of dream-consciousness is itself a case in point. That much of our sleep is pervaded by a mental concomitant none doubts, for the first moments of waking consciousness frequently retain images which have been carried over from dreams and the connection is thus established through memory. But the extent of this correlation and the connection in general which obtains between the physical changes occurring during sleep and activities of consciousness have as yet received only a speculative formulation.

III

Within this group of cases falls a large number of more or less exceptional forms of activity, as well as several types which are essentially normal and important. The exceptional cases are those which, in general, have been described as due to conditions of ' disaggregation ' in the nervous system. They commonly occur only in connection with a definite and highly integrated form of consciousness. They involve complex conditions of reaction and complicated types of response. Frequently their forms are the result of secondary modifications of behavior and represent conventional modes of reaction. They therefore stand for consciousness in the most direct way and we call them, in the narrowest sense of the term, expressions of mental attitude. The occurrence of such responses as the use of the tongue in speech or the hand in writing, for example, is difficult to conceive apart from a mental content of which the product is a specific expression. Yet the condition under which on some occasions these reactions occur raises the problem whether in fact the complex movement had such a conscious correlate or not. In the handling of objects with which the fingers accidentally come into contact while the mind is otherwise engaged, in automatic writing and mimetic movements, in the speech of one who answers in his sleep to a call or question, in the reading which one continues in the face of overpowering fatigue or drowsiness without, for a time, any awareness of meaning or movements, in the speech of hysterical subjects when a so-called secondary self has been evoked,—in all these cases certain similarities of condition exist and a common problem is raised.

These acts are the characteristic expression of our most highly developed forms of consciousness; but in all the cases in question special features exist which we interpret as marking its absence. The subject is in general unaffected by the changes occurring in the world about him, as in sleep; or he has no intention to perform the movement which takes the form of a voluntary act, nor remembrance of it after it has taken place, as in mechanical responses; or his whole significant consciousness seems otherwise engaged at the moment, as in automatic writing and the speech of secondary selves.

The problem of interpretation therefore arises in a special way in connection with all such cases. Shall we conceive the form of response so elaborated that by some provision within the nervous system the impression is drafted off into an adaptive reaction in a way which does not involve the central cortex at all, and that in consequence it is not conditioned or accompanied by consciousness? Or shall we say that a consciousness of both the condition of reaction and the form of response was there but never as the object of engrossing attention and irrecoverable in after-consciousness? Or shall we say, finally, that consciousness is not to be conceived as a unity, being the correlative of certain systematic activities within the nervous mass which do not necessarily involve the system as a whole but may constitute functionally independent and co-existing processes of response; and that a dissociation or disaggregation within the consciousness of the self is possible; two, or a series of such supplementary or split-off consciousnesses thus potentially co-existing in correlation with a single organism? It is a question of interpretation which cannot be answered by an inspection of the type of reaction but must turn upon a consideration of the whole situation involved.

If we accept the first hypothesis, that in such cases the reaction is mechanized in the sense that it is unaccompanied by mental activity in any form, it seems to throw doubt upon the whole process of inferring consciousness from behavior. If we make the second supposition, the function of introspection is invalidated as a criterion in the determination of the problem. If we assert the final alternative it undermines the unity of consciousness and makes the self of psychology a dissolving aggregation of individuals. For in the latter case there is no inherent principle of limitation in the number of socalled selves to be predicated. Any systematic form of response which has attained relative permanence in the complex of functions is sufficient to afford the basis of such a separate self. Thus in every automatic process of the body would theoretically be grounded a consciousness, narrow indeed in its content, but highly integrated and enduring. Under this hypothesis, for example, there would be spinal as well as cervical selves, and within the former field lumbar as well as cervical; for if we are to conceive disaggregation generally spinal consciousness is but a term for the series of complex mental correlates representing the various levels within that system. Man thus becomes a microcosm and represents in himself not only primitive and highly developed forms of reaction but also the correlative series of mental types from protozoan to man.

Thus we need not go beyond the phenomena which normal human experience presents in order to be confronted by the problem of the distribution of mind. It appears in every consideration of bodily activity and presents aspects as perplexing as can be found anywhere within the psychologist's field. Nevertheless it is in connection with mental types rather than mental activities that the question has chiefly been raised. With this form of the problem we have already come into contact. The phenomena of sleep and their relation to the problem of consciousness are matters, it may be said, of the type rather than of any individual bodily activity and its correlations. One may arrange a series of gradations from complete waking life with its vivid and complex mental content to profound slumber with its supposed absence of all consciousness. As one grows drowsy and passes into sleep, mental activity becomes less complicated and its content simpler. The mind slackens in alertness and its self-control is weakened. The mental content becomes disaggregated and breaks of increasing significance appear within its associative processes. The senses are dulled, impressions grow faint and one by one disappear. Finally the sense of self is lost and consciousness vanishes in a dim awareness from which even the last discrimination of subjective and objective has disappeared.

This succession of changes points to a theoretical limit in which consciousness completely vanishes and a purely physical system of changes is presented. Not only in the natural unconsciousness of sleep but also in states artificially aroused or due to abnormal conditions, such as fainting and coma, anæsthesia from drugs and insensibility due to shock or blows, there would then be predicated a field within which no mental correlate attends the physical series of activities which continues to exist.

In these cases, of course, the condition is either exceptional or due to temporary functional relations; but in its more pronounced departures from our own the condition constitutes a characteristic type and reflects profound differences in structural organization. The beginning of this series of types is presented within the human species in two forms. The first is exceptional and consists in abnormal underdevelopment of the organism. In low-class idiots, and, in extreme degree, in anencephalous monsters the general question of the connection of physical activities with consciousness is unavoidably raised. No idiot type, perhaps, justifies a thoroughgoing application of the concept of mechanism, a reaction to stimuli unilluminated by consciousness; but the general want of intelligence, the lack of capacity to receive training, the absence of alertness, the simplicity and fixity of reactions, the rhythmical form which activity presents, not only provoke reflection in regard to the significance of any individual response as an index of consciousness, but mark the idiot as an important intermediate type between full human intelligence and a form of existence in which, from its anatomical incompleteness, consciousness must be absent if the existence of a cerebrum is to be taken as its structural basis.

In human anencephalics the organism is sensitive to a variety of sensory stimuli to which it makes adaptive responses. Food is taken and assimilated. The functions of the great organs are performed and the individual is in every way living and adaptive. But consciousness cannot be predicated even though the organism belongs to that type with which it is most typically connected; for there is an absence of that very structural basis with which, in advancing measure, intelligence is connected throughout the whole animal series.

The second typical departure from the conditions of a complex and integrated consciousness which falls within the human limits is to be found in the earliest developmental stages of the normal individual. The mental content and its forms of integration grow more meagre as we retrace the history of the individual through childhood and infancy to the hour of birth. There the chief part of the objective system of stimuli to conscious activity disappears, so that what content exists during the later uterine period, when the formation of organs has been completed, is incomparably simpler and more vague than after birth. Retracing the process still farther the organic types which the growing organism represents become simpler and more primitive until with the disappearance of the nervous system and the reduction of the organism itself to a mass of undifferentiated cells there is dissolved the whole structural basis upon which the specific attribution of consciousness is made. Either, then, the descriptive content of the term must be given up altogether, or it must be recognized that in the course of these successive transitions there has disappeared that very phenomenon with which psychology is concerned.

Both of these series of types, that which runs through idiocy to the anencephalic condition and that which runs through infancy to the fertilized ovum, lie within the limits of the human species. The third series is that presented by the taxonomic system of living forms. This system, like the former, is characterized by a succession of increasing divergences from the human type as the scale is traversed in regard to both structure and behavior. Concerning those animal types which most closely resemble our own in anatomical features and modes of reaction our judgment is commonly unhesitating. To these, as to other men, a mental life is attributed, and interest centers only in the problem of its specific features and their relation to our own. But as the series is followed farther and the sum of differences increases on the structural as well as the functional side our interpretation hesitates. The system of changes is similar to that which appears as the ontogenetic series is retraced; and the final types with which we are confronted are identical in the two cases, that is, they are single-celled organisms which perform all the essential functions of life but without differentiated structures and in ways that must raise the question whether there is any further value in the inference of an accompanying consciousness,—an inference which is inevitable in the case of the higher species and our fellow men.

In all these series,—taxonomic and phylogenetic, ontogenetic and functional, gradual differentiation and simplification appears, leading at last to radical unlikeness of type. In each case, therefore, the problem of definition and limitation arises and in each case it can receive only a speculative answer. In the attempt to apply this principle of discrimination one must be guided by the whole system of phenomena when no single outstanding feature can be pointed to as the specific correlate of consciousness.

The answer to this problem has been given in terms of four different concepts. The first is that of concomitance between the elementary constituents of mind and matter; the second that of the concomitance of mind with specific grades of material organization; the third that of the concomitance of mind with particular types of adaptation; and the fourth that of the concomitance of mind with certain phases of functioning.

IV

According to the first conception psycho-physical correlation is universal. Mind and matter alike are atomic. As the things of sensible experience are made up of parts and elements, so are individual minds the products of a synthesis of simpler functions and ultimately represent atomic combinations of a primitive mind-stuff. The elements of reality are arranged in pairs, or if the unit of existence be conceived monistically, each monad is at once physical and mental. Every material atom has its own atomic consciousness, each particle of mind-stuff its own body and expression. Under this conception reality is essentially dual. The universal existence of mind as the correlate of matter is posited; consciousness is diffused everywhere in reality. Its existence is a general postulate, not a particular and contingent fact whose distribution is to be determined.

One aspect of the problem is thus eliminated. The inquirer is not called upon to indicate the point at which consciousness appears in the series of organic types nor to assign limits to its occurrence, for it has none. It accompanies all physical existence and change. The essential characteristics of mind must be granted to the simplest forms of existence. Not only will all life be thus marked, but the objects of the inanimate world and its elementary particles as well must be conceived as subjects of feeling and desire, as manifesting individual will.

Though the particular application of this conception in the interpretation of mental content presents evident difficulties, its methodical service is clear. It provides a doctrine of continuity in the interpretation of behavior. It urges the importance at every point of conceiving the simpler forms of life or of action in consistency with the more complex. It affords a way of stating the process of mental evolution by the complication of elementary activities in higher and higher processes; and conversely of treating the functions with which at any stage the psychologist has concretely to deal by resolving them into their theoretical constituents. It provides a metaphysical basis for the assumption of a psychological atomism both in immediate psychical analysis and in the interpretation of ontogenetic and phylogenetic development.

The second conception limits the distribution of psychical phenomena by assuming a specific grade of physical organization as its basis. The criterion of consciousness commonly adopted in this case is the presence of a brain in the organic system. Beneath this level as well as above it extend life and its phenomena; but in the former field its activities are conceived to occur in independence of any conscious correlate. Digestion and assimilation are there, growth and reproduction, even discriminative selection and appropriative reaction, but no consciousness. The adaptations are physiological not psychical; they are tropisms determined by physical irritation and involving only chemical syntheses and disintegrations. But above the line thus drawn the processes in question are conceived to be accompanied by consciousness. Reactions do not simply occur, they are elements in a mental life. The stimuli not only produce responses as before, but they are also sensed as they arise and, in some forms at least, are remembered after they have ceased to act.

The assumption of such a criterion definitely limits the field of consciousness and consequently marks out the bounds within which psychological inquiry may be made if the purpose of that science be, as is commonly stated, to give an account of the phenomena of consciousness and not to describe the behaviour of the various types of organisms. In tracing the outlines of that field of inquiry within which immediate introspection does not suffice, as is the case with young children, persons of diseased minds and the lower animals, the psychologist is thus guided by a specific test, namely, the presence or absence of an encephalon. Where the organic life is conditioned by a brain its phenomena afford subject matter for psychological treatment; where the brain, or the nervous system as a whole, is lacking no interpretation in terms of subjective reality is to be made.

This conception is doubtlessly fostered by our sense of the enormous difference which separates our own conscious life from the behavior of plants and invertebrate animals. The difficulties by which we are beset when we endeavor to treat the phenomena of adaptation in these lower ranges of life in terms derived from the manifold and highly definite activities of our own conscious existence are practically insuperable. In its specific content consciousness is limited to a special field within the region of life as a whole, and the term is emptied of significance when we try to make the two co-extensive.

The division does not fall at the boundary which separates animal from vegetable. Within the former class occur forms of life which, in common sense, must be affiliated with the plant and its reactions rather than with our own conscious life. The protozoa as a group belong here, as do also the colonial metazoa, the coelenterates, the molluscs and perhaps even the worms. The differences which separate these types from the human are of course not completely represented in the absence of a brain; they penetrate the whole body and include sensory mechanisms, muscular tissues and general anatomical scheme. But if we are to single out one special feature to be employed as a criterion no other offers so available a test as the presence or absence of a brain. It is a profound characteristic the existence of which carries many implications. It marks a certain grade of development at large with relative precision. Its existence is characterized by specific forms of behavior which are absent in types lacking such an anatomical feature.

The use of the brain as a criterion of consciousness is a measure in defence of meaning. To make consciousness coextensive with physical existence is to strip the term of all that determinate content which gives it significance when applied to our own life. To retain meaning the term must be limited in its scope, and to restrict its application to organic forms characterized by the presence of an encephalon is to select that physical structure which we find most intimately conditioning our own mental life as the test by which the presence of a life essentially similar to our own is to be determined.

On the other hand there are obvious difficulties to be encountered in the application of such a criterion. As regards behavior the world of life is not divided into two distinct groups marked by specifically different reactions, the cephalic and the non-cephalic. The development of behavior presents no evident breaks. Higher and lower groups are connected by a series of intermediate types which present relative continuity in their relations. Instead of being far withdrawn from each other, the one group passes into the other by insensible gradations. If there be grounds, as regards external organization, for the limitation of the field of consciousness, it cannot be said to have its justification in any sharp delimitation of two classes of animal behavior associated with the two organic types respectively.

Further, if regarding behavior as the objective manifestation of organization we turn to the question of structure the same lack of demarcation will be found there also. The world of life is not divided into two isolated and contrasted groups, one possessing a brain and the other lacking it. The cephalic type presents a range of variations marked by very great differences in complexity and integrity of organization between its highest and lowest members. The non-cephalic group, likewise, includes such extremes as the complete absence of nervous elements in the protozoa and the high development of the segmented type in the vermes. Moreover, the two groups are in immediate contact at their edges. No anatomical chasm separates the two, such as divides, for example, any two species separated by intermediate types. When followed downward the cephalic type is found to disappear in forms difficult to classify. Indications of a brain appear in the swelling of the foremost nerve-bulbs, but so slightly do these changes alter the essential type found in the upper grades of the group below that classification may even be said to turn upon the relations in which the type in question is conceived. One who is tracing the development of the segmented type of nervous system will see in it a non-cephalic structure; one who is following the development of the brain will find in it the progenitor of all higher cephalic forms. It is an intermediate type.

The rise of the evolutionistic point of view has given a new meaning to all such intermediate forms. These types do not, as for earlier systematists, simply obscure the problem of classification; they mark a genetic, not merely a logical, relationship among the types which they connect. Even when breaks are found, therefore, continuity must still be assumed. The cephalic type has arisen by successive modifications from the non-cephalic, and the higher of the non-cephalic from the lower in unbroken succession. A fundamental protoplasmic continuity thus unites the highest cephalic forms with the lowest and wholly non-nervous types of organic life.

The psychological difficulty which attaches to such a division of the world of life into conscious and non-conscious groups appears in its full bearings only when one turns to the phenomena of embryonic development. Whatever may be said as to the magnitude of the differences which separate proximate forms in the evolutionary series and the significance of the mutation theory, the young of any member of the higher or cephalic group at least passes by a continuous process from the non-cephalic to the cephalic type. The earlier form of organization is not replaced by a later and disparate structure but is itself transformed into it by insensible modifications in the course of a continuous life. Here, therefore, the division becomes merely a convention which is useful in marking stages of organic development separated by differences of a certain magnitude but implies no interruption of continuity such as the distinction at first sight seems to imply.

Within this second category apparently fall various attempts to express psychical activity in terms of relational concepts derived from the physical sciences. The material phenomenon in which attention centers here is that of change or energy. One may conceive the relation as universal and interpret all physical activity in subjective terms and say that every manifestation of force is the expression of purpose and will. The distribution of mind is thus made co-extensive with that of matter, since all material existence is manifested through resistance. The distinction which is made between motion and resistance must then be within the general field of subjective experience; it cannot be a delimiting concept which makes will correlative with motion, leaving resistance outside the field of subjective phenomena. The two forms of force can only be differentiated in some such terms as offence and defence, as aggressive and self-preservative aspects of the universal psychical.

In this form the theory differs from that first described only by the modification which appears in the fundamental concept of the physical world. If mind be the correlative of all physical change,—the subjective aspect of energy—then all material existence possesses such an aspect. No complex of substances lacks it, since all compounds must be described in terms of a system of forces; and no atom of matter can exist in separation from it, unless we say that in so far as the atom is not impinged upon its resistance is only a potentiality, the concept receiving a positive content through the conflict with other atoms. We should thus have to speak of the sleep of the atom and its awakening through the occurrence of stimulation. If the atom be defined as an hypostasis of energy and nothing more, as simply the metaphysical bearer of motion, then no such distinction would be in place. The system of subjective experience would be a thoroughgoing correlate of the system of energy which the physical world represents.

Within this general field various modifications of the concept in question appear. In one of these consciousness is conceived as a special form of energy, one of many, each of which has its own specific characteristics. Light is such a special form of energy and heat, electrical activity and chemical affinity. Among these appears consciousness, qualitatively unique as is each one, but of the same order as the rest. Under this conception the distribution of consciousness is limited and particularized. Energy as such does not manifest it, but energy only as it appears under special conditions and relations. One mode of motion is manifested as light, another as heat, a third as consciousness, a fourth as electricity, and so on. The appearance of consciousness is thus conditioned by the existence of certain specific forms of physical activity.

But all differences in physical processes are associated with determinate variations in structure. This conception may therefore be said to constitute a modification of that which bases the determination of consciousness upon the existence of a certain type of material organization.

In still another form of this concept consciousness is made the subjective aspect of latent energy. Under certain conditions motion disappears and is dissipated, as when a spring is released or a stone drops to the earth. In other cases motion disappears but is not dissipated. It remains available for future use and may be retransformed into kinetic energy, as when a spring is compressed or a stone reaches the highest point of its ascent when thrown upward. The capacity of a compressed spring or a lifted weight to do work is called latent energy. This condition of latency in physical energy, according to this form of the criterion, is what we know as consciousness. Wherever latent energy appears, therefore, consciousness must be posited. It is thus made a characteristic not of nervous matter or living substance but of all grades of material organization and may appear equally in a stone and in a man,—equally, that is, in respect of its formal existence, not of its specific content. For as one condition of stress must be treated as complex and another as simple, so also must consciousness, or latent energy, present analogous modifications in composition.

V

The third criterion looks primarily to reaction but involves a reference to anatomical bases as well. Consciousness is here conceived as the concomitant of general or specific modes of response to the environment. The general forms of behavior, as well as the specific types of reaction, which characterize the different organic forms vary within very wide limits. The specific reaction-types depend for their form upon the nature of the stimuli to which the organism responds and the systemic features which condition reaction. Sense-organs and muscles provide the material basis upon which the forms of animal behavior are founded, and variations within these systems of mechanisms make very great, and in certain ways characteristic, differences in the organic type.

But to complete the description of behavior a second group of facts must be included, for behavior is a question of the uses to which the materials afforded by sensations and muscle complexes are put. The range of movements may be wide or narrow; the forms of response to a given stimulus may be highly defined or diffuse and obscure. The reaction which is called forth may be fixed or variable, simple or complex, confined to a single form of muscular adaptation or resulting in any one of the whole series which the structural plan makes possible. These differences in the general character of the response which the organism is capable of making mark the distinction between high and low grades in the scale of behavior. That reaction is superior which is highly defined, complex and variable,—in other words, which shows modification in response to characteristic differences appearing among the system of stimuli. That organism is high in the general scale of behavior which is marked by the capacity for a wide range of stimuli discriminations and of differing muscular reactions on the one hand, while on the other its responses present refined modifications corresponding to variations in the specific conditions of reaction.

These features of the organism's adaptive response are of value not merely as means by which behavior may be characterized, but also because they may be used as a criterion of the distribution of consciousness as a concomitant of specific forms of behavior. Introspection reveals certain evident connections between these two things. Our simple, highly defined and relatively fixed reactions are accompanied by a form of consciousness which is at least commonly obscure, if consciousness can be asserted at all as their uniform concomitant. It is those experiences in which the response is complex, diffused or subject to great variation which are typically and intensely conscious. Most of all is consciousness characteristic of situations in which a definite response has not yet been established, or is not at the moment discoverable. With the progressive clarification of the situation,—that is, with the full discrimination of the system of stimuli involved—and the establishment of a more and more direct and fixed connection between the situation and its interpretation,—that is, with advance in the direction of setting up an immediate adaptive response to the stimulus—consciousness declines. In the most highly defined forms of reaction, even when such development represents an acquired modification, consciousness has ceased to be a discernible accompaniment. The type of such highly automatized responses is to be sought in the most simple and fixed forms of reaction, whether acquired or congenital, voluntary or involuntary, modifiable or unmodifiable.

If we take this series of facts as our guide we shall find ourselves recasting the conditions of consciousness in terms of the complications which appear in the form of animal behavior. A certain grade of complexity, or a certain degree of variability, in the response will now become the criterion of its presence. A variety of modifications in this concept is possible. The criterion may turn upon the inner wealth of discriminable stimuli and the complexity of the elements of possible response. The form of human consciousness is closely related to the rich manifold of stimulations to which the human body is susceptible, and to the variety of motor reactions which its structure makes possible. Where, then, the organism presents a certain range of stimulations and variety of elementary reactions we may say: here is the necessary background against which consciousness is to be projected, from the existence of which its presence is to be construed.

With the same general situation in mind, namely the inner richness of the elements of stimulation and reaction, attention may, in the next place, turn to the nature of the connections which exist between the specific classes of stimuli and the particular reactions which they prompt. In one case these connections may be in a single direction,—as when a local reflex occurs or a peristaltic movement takes place—in which case the association between stimulus and reaction will be highly definite and fixed. In another case the connection may be a multiple one, each specific stimulus being capable of calling forth not a single limited reaction but any one of the whole series, made possible by the structure of the organism. In such a case the stimulus has no fixed line of discharge. The field of its potential effect is the whole system of activities which the organism represents. It may characteristically result in a single and direct discharge, in which case it will be least conscious. (If the discharge be exclusively into the one reaction system it will theoretically be unconscious.) It may result in a discharge accompanied by diffusion of excitement over a larger area, in which case there will be a reaction accompanied by consciousness,—a reaction apprehended or intended. It may result, finally, in a general diffusion without specific reaction, the whole group of associated reaction-systems taking up the shock of the stimulus and absorbing it without localized response. What this situation presents is not, strictly speaking, the absence of all tendency to reaction but the summation of all such tendencies, which at the instant are so related that their combination produces a state of suspension,—in other words, the content in such a case is to read positively not negatively.

It is these cases which are typically suffused with consciousness. They constitute the experience of hesitation, doubt, deliberation in so far as their relation to adaptive response is concerned, and that of supplementive association, of apperception or interpretation of the situation in so far as relates to the stimulus.

This conception, it is clear, involves a reference to the structural complexity of the nervous system and cannot be isolated from the grade of organization which is thus assumed. But every criterion based upon reaction type involves the same reference, while it is, of course, true that the phenoma to which attention is directed in all these cases is not the system of anatomical features as such but the type of behavior which it manifests. In this form of test the central element is the variability of the reaction to stimuli.

This appeal is seen in another and perhaps more complex form of behavior criterion, namely in the insistence upon progressive modification of reaction in response to changes in the system of stimuli. It is more complex because it turns to a further aspect of variability as the criterion of consciousness. The whole inner richness of stimulation and elementary reaction may be assumed and the internal diffusion of the stimulus over a wide system of elements together with the conditions of its transformation into any one of a multitude of different responses may be given, yet one may still conceive the absence of a conscious correlate. The number of the elements and the complexity of their relations are not sufficient, if variability be made the controlling idea in our conception. For even in a complex organization, such as is here imagined, equilibrium may exist in the sense that the system is capable of attaining a condition of stability in its relations. In such a case the stimulus must be conceived as taking a definite and habitual, though highly complex, course of irradiation, a given stimulus-type producing on each occasion the same wave of redistribution within the group of elements. The system of things will be complex but not necessarily varied. The organism may therefore be regarded under the conception of automatism and its reactions described in terms of purely physiological tropisms.

VI

In the face of this theoretical possibility a further phase of variability is seized upon: consciousness shall be predicated only in those cases where the whole system is subject to modification in response to progressive changes in the stimuli which act upon the organism. In other words, the capacity to learn from experience will be made the criterion of consciousness. If the learning process itself be the point to which the mind turns in this analysis the criterion will be called the modifiability of behavior; if it be the influence of experience in such modification it will be termed organic memory or the like.

The advantage of this last group of conceptions is evident. In their application the field of discussion shifts from theoretical to practical considerations, so to speak; the evidence is no longer secondary and derivative but primary and immediate. That is to say, the criteria now appealed to are no longer abstract or technical, but just such as we use in our general human interpretations. The universal basis on which consciousness is popularly predicated is that of conduct or behavior, not of organization or anatomical type which, though confessedly the substrate of behavior-modes, is largely removed from direct observation and revealed only through the slow progress of descriptive science.

The starting-point of all such attribution of consciousness in connection with other organisms is our intuition of the expression of mental states by characteristic reactions in our own experience. Upon the felt identity in behavior proceeds our inference of likeness in the psychical correlate; and, far as this inference may be pushed, it is still in practical judgment dependent upon the observation or assumption of a conduct basis.

In the conception now described this characteristic is made the systematic criterion of the distribution of consciousness in the organic world. If the behavior of the type in question is essentially like our own, or at least like those simplest forms of reaction with which in our own immediate experience we know consciousness to be associated, we conceive the life of such an organic type to be characterized by a conscious correlate. The difficulty in applying this criterion lies in the function of interpretation. The complexity or variability of behavior is really an inner complication of the situation, not an immediate and obvious feature of the reaction objectively considered. A simple muscle contraction, from the point of view which must be taken here, may be a most complex act. Delay in the response to a stimulus,—the very failure to appear itself,—must often be construed positively as a real and highly developed reaction. Every response must thus be given its full and proper setting in the dramatic reconstruction of the situation under analysis; and in this synthetic representation the observer may err either by ascribing to behavior a complexity which it does not really involve or by reducing it to terms of too great simplicity. Comparative psychology has wavered between thinking the brutes human and making them automata. The popular conception of animal life undoubtedly errs by reading into its reactions a complexity which the evidence does not justify; while recent psychology has, in some cases, adopted a schematism which reduces higher forms of behavior to chains of simple and unmodifiable tropisms.

The question at issue, however, is not as to the identity in structural formula of tropism, instinctive response and purposeful action, but of the presence or absence of variation in the inner complexity of the organism as the subject of reactions. It seems on the whole likely that we shall have to modify our conception of the simplicity of behavior in even the most elementary organisms and ascribe to them a selection and initiative, a variety and complexity of reaction for which the theory of tropisms does not make sufficient room.

The final conception, which should not perhaps be given a separate place, makes consciousness neither the necessary correlate of a certain grade of organization nor yet the uniform accompaniment of reactions of a certain complexity. It maintains, on the contrary, that there is no necessary correlation between the type of structure or function and consciousness, that the latter appears and vanishes in connection with even the highest forms of behavior. It is an evanescent phenomenon which hovers over the physiological process only during certain phases of adaptation, namely those stages in which the response is uncertain and hesitation arises as to the reaction to be made. Consciousness thus appears as the mark of imperfect adaptation. In those organic forms which are characterized by few and simple relations with the environment adaptation may reach a high degree of determinateness, and consciousness will then scarcely rise above the vanishing point. As the multiplicity of relations increases and the complexity of the conditions which each reaction represents grows greater the field of consciousness will develop. The range of reaction alternatives which a situation presents to any individual or organic type is the measure of uncertainty in its responses. When life is constituted of a succession of situations every element of which presents inner complexity, then novelty, doubt, choice, striving and reconstruction will never be absent. Consciousness under these conditions will therefore be continuous and pervasive, variations arising from the swing of its focus from point to point in the system of correlated stimuli and reactions.

While consciousness may be called self-eliminative in the sense that it is the characteristic accompaniment of those processes in which an adaptive reaction is in course of establishment through progressive selection,—the result of which is decline and eventual elimination,—its disappearance from the system of changes never actually results, since new points of friction constantly appear, thereby providing the basis of a continuous mental activity. The sources of these novel occurrences must be stated in terms of two systems of factors. The first of these is the persistent appearance of new elements and combinations in the stimuli upon which the organism is called to react, which,—even in the case of the most highly organized habits,—call for incessant refined modifications in the form of behavior. The second set of factors is represented in what may be called the margin of attention. Adaptive activities have their roots in the organism as well as in the environment. The former construes the environment in terms of its own needs and purposes; when, therefore, any one element has been brought within its systematic group of reactions the energy represented in its assimilation does not lapse but, in the human species at least, is turned in new directions and engaged in fresh processes of interpretation and application. The establishment of any one habit, therefore, merely provides the opportunity to turn attention to some new problem of adaptation; one form of habituation gives place to another in endless succession. Adaptation thus approaches no fixed theoretical limit; the field of its future application is extended by each advance that is made. With the progressive refinement of adaptation which marks the transition to higher and higher grades of organic complexity consciousness therefore becomes more keen, pervasive and continuous.

This concept is, however, but a further modification of variability in behavior as the criterion of consciousness. It is an insistence, sharper and in greater detail than heretofore, upon the modification of reaction in all its phases and relations as the essential feature with which consciousness is to be connected. Not to be able to learn but *to be learning* marks its presence. It presents, therefore, no formal advantages or difficulties which are not found in the preceding group.

In the application of any such criterion a distinction in the content of the term 'consciousness' must be noted. In one sense it denotes the unity of functions which constitutes individual experience. Each psychical activity is a function in a personal life. Conceived in this way consciousness stands for an irreducible reality. Each particular process or function is but the unitary conscious life in this phase of its activity or in that, seen now in relation to one condition or object then in relation to another. Each activity must be interpreted in terms of its relation to this unity. It has no independent status and cannot be conceived except as a member in such an integrated system of significant relations.

But there is another way in which this same life may be treated. It is a complex of phenomena as well as a unity of functioning. The specific relations among the constituent activities are subject to change; the particular constituents themselves vary. The formal scheme of their connections and sequences is therefore subject to incessant modifications. Every element and succession of this unitary mental life has a definite character and place in the system of phenomena as a whole, and the working out of their relations and laws constitutes a problem for descriptive or explanatory treatment.

It is this alternative point of view which determines the method of science. Its field of inquiry is uniformly conceived as a complex of phenomena whose correlations are to be worked out. Whether it be material changes or psychical events makes no difference. It is the nature and relations of the constituents, in the one system as in the other, with which science is concerned. Under this assumption the mental function must be isolated in thought and treated as though it were discrete. It is not memory as a phase of the self's activity with which psychology deals but memory as a system of phenomena having this relation to the facts of perception and that relation to the facts of reasoning. It is memory as depending upon this type of imagery in one individual or class of individuals and upon that in another. It is memory in its association with past impressions or future reactions; memory as it enters into imagination and conditions reflection; memory as it develops and declines in the course of individual history,—which the psychologist studies. The same holds true of course for every constituent of the mental life. The latter terms stands simply for this system of related elements, a system which presents a vast range of change both in its constituents and in their relations as the whole field within which the existence of mental phenomena is assumed passes in review.

When, therefore, we speak of the distribution of consciousness it is the complex of processes which we must have in mind, not the unity of functioning. The problem should properly be restated in terms of individual functions and their occurrence. The question is thus particularized. The psychologist does not ask what the distribution of consciousness as such may be, but how wide the occurrence of memory is, what the limits of abstract reasoning are, what organic forms share the life of imagination, at what level moral feeling arises, and the like.

For the formally single question as to the distribution of consciousness in the organic world is substituted a group of questions concerning the several individual functions and their appearance in the scale of life. Under this conception the problem is not where a line shall be drawn separating the organic world into upper and lower halves, conscious and unconscious, but how we shall describe that world in terms of a series of complications giving a succession of levels in the synthesis of psychical elements and processes, such as Sentience, Constructive Perception, Recognition and Memory, Productive Imagination, Abstract Reasoning, and the like.

It is thus the concept of consciousness which undergoes modification as the scale of life is passed in review. The unity of functioning is assumed equally at all levels, whether the component activities be manifold or meagre. The simplest group of activities will mediate it. Were the functions reducible to a single process that unitary life would pervade it, and the greatest complication of its qualitative content or form of organization in no way modifies its essential unity.

We are not called upon under this conception to construe the qualitative modifications of this unity intuitionally. We may frankly confess our inability to conceive what it feels like to be a fish or an oyster, a sea-urchin or an amoeba. Our problem is the simpler one of evaluating evidence, at each successive level of physical organization and in connection with each complication of the reaction system, for the existence of individual constituents in the complex of mental activities at large.

ADAPTATION AND THE CHEMICAL THEORY OF SENSORY RESPONSE

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I. Hering's Theory of the Metabolism of Response

It is the purpose of the present paper to offer certain amendments to Ewald Hering's theory of the metabolism of response.[1]

This theory, which has been applied by its author principally to the visual and temperature senses, is based upon a consideration of the states of equilibrium and disequilibrium which exist under different conditions between the opposed vital processes of anabolism and katabolism. It is obvious that in a stable organ or organism these processes of constructive and destructive chemical change must approximately balance each other, and when this state of balance is determined by the forces of the living body alone it is called by Hering "autonomous equilibrium."

However, a living body is very seldom unacted upon by outside forces and if such forces cause response this must be because they influence either the constructive or the destructive phases of the metabolism. If a certain constant stimulus increases the velocity of the destructive chemical process it will tend to decrease the amount of destructible chemical substance in the body or organ upon which it is acting. In doing this it will also cause a relative diminution in the amount of such substance which can be destroyed under the influence of the stimulus in a unit of time. In other words, stimulation will diminish the sensitivity of the organ. It follows, moreover, that the rate of the destructive change will decrease constantly until—with the stimulus still acting—it is again exactly balanced by the unstimulated constructive change. This state of affairs is what Hering calls "allonomous equilibrium at low potential."

It is clear that an entirely analogous line of reasoning will follow from the conception of a stimulus whose effect is to augment the constructive rather than the destructive phase of the metabolism. The equilibrium which is finally reached under the action of such a stimulus is called by Hering "allonomous equilibrium at high potential."

The chemical condition of an organ in allonomous equilibrium differs quantitatively from that of an organ in autonomous equilibrium. If the allonomous equilibrium is at high potential there is an excess, or if it is at low potential there is a deficiency, of destructible substance in the organ, as compared with the amount characteristic of autonomous equilibrium. When a stimulus is withdrawn after a state of allonomous equilibrium has been reached the organ tends naturally to return to the autonomous level, thus reversing the chemical motion which was brought about by the stimulus. All motion of this sort is change from one state of equilibrium to another and is itself allonomous or autonomous according as it is determined by the stimulus or by the internal vital forces, respectively.

Hering's theory of the psycho-physics of response supposes that sensation is conditioned by definite changes in the equilibrium state of the appropriate sense-organ.[2] A stimulus causes sensation by disturbing the equilibrium and thus inviting the metabolic process to seek a new level, but when the latter is found sensation vanishes. When the stimulus is removed the living substance automatically returns to its original state, and during this process, also, there is sensation, usually of a type opposite in quality to that caused by the stimulus. Such shifts in the point of equilibrium always imply alterations in the sensitivity or "potential" of the living substance.

II. The Criticism of Hering's Theory

The theory of the metabolism of response above outlined has some remarkable merits and almost as many remarkable demerits. On the credit side we may count its power to explain the adaptation of sense-organs to specific stimuli, the phenomena of after-images, and the relation of antagonism which exists between different stimuli in certain departments of sensation.

However, Hering's theory has been justly subjected to severe adverse criticism.[3] In the first place, a large part of its explanatory power depends upon the supposition that stimuli encourage anabolic as well as katabolic processes in protoplasm. The arguments which have been brought forward against this supposition are more general than we should wish, but they are nevertheless fairly convincing. Anabolism is essentially a repair process, a restitution initiated by internal forces to compensate for the destructive effects produced in the organism by stimuli.

There are other more serious objections to Herin's views. One of these is found in the fact that, contrary to the demands of the theory, continued constant stimulation does not invariably end in the disappearance of sensation. Moreover, the present writer has shown experimentally that, at least in the case of vision, although changes in the equilibrium point occur during the process of stimulation, by far the greater part of our seeing must go on under conditions which are approximately those of stable equilibrium. In addition, Hering's theory may be correctly accused of formalism. It takes too little cognizance of the concrete concepts of modern chemistry and physiology, which readily fit into the general form of the theory and at the same time render it more realistic.

III. A Revision of the Chemical Response Theory

It is impossible to seriously question Hering's fundamental postulate, the statement that organic matter is characterized by concomitant destructive and constructive chemical change, and that active organic response must be determined by an augmentation of one or the other of these processes, under the influence of the stimulus. If chemical change occurs it must consist in the synthesis or in the decomposition of some substance, and this substance must have a certain definite *concentration* in the living matter which is being considered. Let us designate this substance by S, and its concentration by s. For convenience we shall confine our attention to the processes which occur in a single living cell, and we shall define s as the absolute quantity of the substance S which is contained in the particular cell which we are to study.

Now since we intend to regard the katabolic phase of metabolism as the only one which is directly affected by the stimuli which act upon the cell we may simplify the general mechanism concerned by supposing that no synthetic changes at all occur within the cell, but that the substance S is supplied to it ready-made, by the blood or lymph. From a formal point of view this *nutrition* of the cell is equivalent to a synthetic process going on within it, since it tends to bring about an increase in the concentration of the substance in the cell. Accordingly, it can be represented by the derivative, ds/dt, the rate of change of the concentration of S with respect to the time. The katabolic changes within the cell are also representable by a similar derivative, with negative values. For purposes of abbreviation let us write:

$$ds/dt = \dot{s} \tag{a}$$

Now we must suppose that in an undisturbed cell which is metabolically stable the processes of nutrition and katabolism are equal and opposite. Hence, if the rate of nutrition is a, and the rate of katabolism k, we have:

$$\dot{s} = a - k = o; \tag{1}$$

which may be designated as the *equation of rest.* This equation does not mean that the living substance is inactive in the rest condition but merely that its activities are balanced in such a way as to produce no resultant or average change in the concentration of S. The physical chemist expresses practically all states of apparent chemical quiescence as equilibria of this general sort.

It is in accordance with common-sense, as well as with the law of "mass action" in chemistry, to suppose that the rate of "autonomous" decomposition, k, of the substance S will depend upon the amount of S which is present to decompose, that is, it will depend upon the concentration, s. If the relationship in question is a purely chemical one, it will be a proportionality,[4] so that:

$$k = c_k s, \tag{2}$$

where c_k is a constant, the "reaction" which is concerned being of the so-called *irreversible* type and hence being independent of the accumulation of waste products. We shall speak of this expression as *the equation of self-excitation.*

Now, in order to avoid the difficulties encountered by Hering, let us assume that the stimulus, I, always produces a *katabolic change* in the substance S, and has no direct effect either upon the rate of nutrition or upon possible anabolic processes. For simplicity's sake we will make the probable assumption that the increase in the rate of katabolism engendered by the stimulus is proportional to the intensity, i, of the latter, so that if this increase is d, we have:

$$d = c_d i s, \tag{3}$$

which may be called *the equation of response.* d is proportional to s for the same reason that k is so proportional (see equation (2), above).

Thus, when a stimulus is acting, we have, from (2) and (3) and the definition of $\dot{s}$:

(b) $$\dot{s} = a - k - d \qquad \text{or}$$

(c) $$\dot{s} = a - c_k s - c_d i s \qquad \text{or}$$

(4) $$\dot{s} = a - (c_k + c_d i)\ s$$

This will be known as the *equation of total activity.* The values of $\dot{s}$ which it includes are, of course, in general not the same as those of (1), above.

The term, a, represents the repair, while the term, $(c_k + c_d i)\ s$, represents the destructive process. The first part of the latter stands for what Hering would call autonomous katabolism (or dissimilation), the second part for what he would designate as allonomous katabolism. a is, of course, autonomous.

Now it is obvious that if the second term, above, is larger than the first, $\dot{s}$ will have a negative value, and since $\dot{s}$ is the rate of change of s, this means that s will constantly decrease. But as s decreases the term $(c_k + c_d i)s$ must decrease also, and this change will obviously go on until the last mentioned term becomes equal to a, at which time $\dot{s}$ will become zero, and s will thus cease to vary. In this way there will be established an equilibrium analogous to that represented in equation (1)[5] but involving a stimulus intensity, i. It corresponds, then, with the allonomous equilibrium at low potentid of Hering's theory.[6] Since by definition of an equilibrium state we have: $\dot{s} = o$, for such an allonomous equilibrium we can write:

(d) $$\dot{s} = a - (c_k + c_d i)\ s = o \qquad \text{or}$$

(5) $$s = \frac{a}{(c_k + c_d i)},$$

the equation of equilibrium sensitivity, which expresses the concentration of the substance, S, in terms of the intensity of the stimulus, I, and certain constants.

It is obvious from the form of equation (5) that the greater the intensity of the stimulus the lower must be the concentration of the substance, S, at equilibrium. Since by equation (3) the degree, d, in which the living substance responds to a stimulus of intensity, i, is proportional to s, s may evidently be appropriately defined as the *sensitivity* of the sense-cell. *Hence we arrive at the general result that the higher the intensity of a stimulus which produces allonomous equilibrium, the lower the point to which the sensitivity of the organ is* *reduced.* This result is in accord with the facts of response in many departments of sensation.

Now, in accordance with equation (4) the katabolic component of the process within the sense-cell is always:

$$q = (c_k + c_d i)\; s \tag{6}$$

and since by the definition of an equilibrium state,

$$q = a, \tag{e}$$

it follows that under the assumptions thus far made the rate of the katabolic process at any allonomous equilibrium is independent of the intensity of the stimulus responsible for that equilibrium, and is, accordingly, the same as that at autonomous equilibrium.

The question may now be raised as to whether the repair process, a, actually does remain constant during stimulation. It is in harmony with our general knowledge of vital behavior to suppose that marked destructive changes in the substance of a sensory cell will be accompanied by a *positive increase* in the restitutive process. As a matter of fact, the physiological situation which is involved in the process of nutrition makes it easier to assume that such an increase occurs than to support our previous, mathematically simpler, assumption of a constant repair.

A cell is a portion of space marked off from the lymph in which it is immersed, by a membrane. Through this membrane the sensitizing substance must pass. It is probable that if the molecules of the substance in question can pass into the cell they can also pass out. Since the process under consideration is essentially one of osmotic diffusion the rate at which the substance is penetrating the cell at any instant must depend on its concentration in the lymph, while that at which it is leaving the cell must be proportional to its concentration inside.[7] The net rate of increase in concentration of the substance in the cell will, of course, be the difference between the income and the outgo, and this difference is identical with the a of our equations. Hence, if we suppose the concentration of the substance, S, in the lymph to be a constant we can write:

$$a = r - c_r s, \tag{7}$$

the equation of compensation, where r is the rate of income, and $c_r s$ is the rate of outgo. s, of course, is the concentration of S within the cell. Under the conditions, $c_r s$ must always be less than r.

It is obvious that if this equation is true the rate of nutrition will not be constant, but will increase as the sensitivity, s, of the cell decreases. If we substitute the above value of a in equation (4) we get:

(f) $$\dot{s} = r - (c_r + c_k + c_d i)\, s$$

or, for equilibrium:

(8) $$s = \frac{r}{(c_r + c_k + c_d i)}$$

(The equation of equilibrium sensitivity for compensating response) an equation which has the same general form as (5).

If we now substitute this value for s in equation (6), we have

(9) $$q = \frac{r\ (c_k + c_d i)}{(c_r + c_k + c_d i)}$$

(The equation of equilibrium excitation for compensating response) which states that the rate of the katabolic process at allonomous equilibrium is higher the higher the intensity of the stimulus, i being the only variable on the right-hand side of the equation. From the form of equation (9) it is obvious that q must always be less than r. Equations (7)-(10), inclusive, characterize what may be designated as a *compensating response.*

Expression (9) holds for autonomous as well as for allonomous equilibrium, and for the former since: $i = 0$, the expression reads:

(g) $$q = \frac{r\ c_k}{c_r + c_k}$$

The term on the right-hand side of the equation may be described as the *non-compensative response,* since it is natural to say that the compensation is zero when the stimulus is zero. It is thus possible to define the *compensation* itself as the difference between the non-compensative and the actual response under a specified stimulus, i, as follows:

(h) $$u = r \left(\frac{c_k + c_d i}{c_r + c_k + c_d i} - \frac{c_k}{c_r + c_k} \right)$$

When $i = 0$, $u = 0$; when i is very large $(= \infty)$ $u = r \left(1 - \frac{c_k}{c_r + c_k} \right) = \frac{r\ c_r}{c_r + c_k}$ so that the compensation can vary only between the limits, 0 and $\frac{r\ c_r}{c_r + c_k}$, the latter term representing the greatest difference in excitation which can exist between an autonomous and an allonomous response under equilibrium conditions.

Now whether or not a given process of response appears to be compensative must depend upon whether or not the value of u at equilibrium is sufficient to produce a *threshold difference* between the self-excitation and that brought about by the stimulus. In other words, if Δq is the difference limen in question, in order that the response should appear compensating the relation $u > \Delta q$ must hold. If $\left(\frac{r\,c_r}{c_r + c_k}\right) < \Delta q$ the response will always appear to be non-compensating.

When any stimulus whatsoever is applied to a sensory cell in allonomous equilibrium the excitation first rises, and then begins to fall; and it will drop finally to an equilibrium level which is determined by the intensity of the stimulus itself, in accordance with equation (9). The excitation under such conditions of allonomous equilibrium must always be considerably less than that which was produced by the stimulus at the outset. Thus, while a stimulus may at first cause a sensation which differs appreciably from that which is characteristic of the self-excitation of the sensory system concerned, adaptation may so diminish this difference that it becomes, at equilibrium, wholly indistinguishable. When such is the case the response will be to all intents and purposes non-compensating. For a higher intensity of stimulation, however, the equilibrium excitation will be greater, and hence for stimuli of this description the compensation will be apparent. Accordingly, we may argue that in all departments of sense in which the chemical mechanism which we have been considering is active, evidences of the existence of compensating response are not to be expected for relatively weak stimuli. Just how strong a stimulus must be in order that compensation should appear in connection with it must depend upon the magnitude of the difference threshold for the sense under consideration, and upon the nature of the constants in equation (h). It is obvious from the form of the equation in question that for $c_r = o$ the compensation will be zero. The larger c_r the greater will be the compensation for a stimulus of given intensity.

Equation (9) asserts that the excitation at allonomous equilibrium increases as the intensity of the stimulus increases, but the relation which is stated to hold between the two is not a proportionality but an asymptotic function more or less logarithmic in form. Consequently *equation* (9) *may be regarded as a statement of Weber's law in so far as this law applies under equilibrium conditions.* However, it is the writer's opinion that the Weber relationship—which is known to be only approximately logarithmic—holds not only between the intensity of the stimulus and that of the excitation in the receptor, but for every transition in the sensory-cerebral system.

Let us now consider the somewhat more difficult problem as to the exact process which occurs when a stimulus is applied to an organ whose substance is in autonomous equilibrium. For simplicity's sake we shall confine ourselves to the case of non-compensating response, but the results will be typical of compensating response as well. In accordance with equation (3), when a stimulus, i, is first applied to a sense-cell in autonomous equilibrium it increases the rate of katabolism by an amount: $d = c_d is$, if s is the concentration of the substance, S, under the conditions of this equilibrium. The equilibrium itself is thus destroyed, so that s decreases, and with it d, and a new *allonomous* equilibrium point is reached, the conditions of which have been described above.

The first result which appears, then, is that the effect produced by the stimulus is greatest in the beginning, decreasing relatively rapidly at first, more gradually later on, until it finally reaches a level at which it is constant. In mathematical language, the effect produced by the stimulus decreases *asymptotically* towards an equilibrium condition determined by the intensity of the stimulus.

By use of the calculus it is easy to derive from the above equations the following expression, which shows how the rate of the katabolic process in the stimulated cell varies with the time:[8]

$$(10) \qquad q = a + \frac{1}{\mathbf{e}^{(c_k + c_d i)t}}$$

where **e** is the base of the natural logarithms, and t is the time which has elapsed after the application of a stimulus of constant intensity, i. Examination of this function shows that as time goes on the value of q approaches a as a limit, but theoretically would only reach it after an infinite period. If we put: $t = \infty$, the term containing it becomes zero, so that: $q = a$, which is obviously the condition for equilibrium. As with all curves of this sort, the rapidity with which the limit is being approached is less the greater t, and in the region of very large values of t is practically zero.

Expression (10) may be named *the equation of adaptation.*

In a similar manner we can calculate an expression which shows how the autonomous katabolism will increase after the stimulus is removed. The exact form of this expression varies, of course, with the value of s which corresponds with zero time. It is simplest if the value selected is: $s = \frac{a-1}{ck}$, when it becomes:[9]

$$q = a - \frac{1}{e^{ckt}}, \tag{11}$$

the equation of recovery. It is obvious that for small values of t the change in the term, $\frac{1}{e^{ckt}}$ will be relatively rapid, while for large values it will be relatively slow. After the passage of an infinite time the term becomes zero, so that, once more, $q = a$, the condition for equilibrium.

Accordingly, the recovery process, like the process which accompanies stimulation, is asymptotic. The sensitivity of the living substance increases rapidly at first, and then more slowly until autonomous equilibrium is practically reached.

Equations (10) and (11) have been calculated on the assumption that the repair process is constant, as represented in (1)-(7), above. If, on the other hand, equation (7) is introduced into the calculations, expressions will be obtained which differ from (10) and (11), but not in an essential manner.

IV. Interpretation of the Theory

It is obvious that the equations derived above offer an approximate representation and rationalization of the processes of rest, stimulation, fatigue and recovery from fatigue, which occur in various sense-organs. In our subsequent discussion *we shall regard sensation as dependent upon the intensity, q, of the katabolic process at any time.*

Certain of the senses show what may be called *self-activity,* as illustrated, for example, by the familiar "idio-retinal light" in the case of vision. The generic law of such spontaneous excitation is given in equation (2). The apparent absence of this unstimulated activity in some sense departments may be accounted for if we suppose that the value of c_k in these departments is so small that the corresponding sensation falls below the threshold. Another possible explanation for the seeming lack of unstimulated activity may be found in the fact that in many cases an *antagonism* exists between two or more simultaneously active sensory mechanisms, so that the unstimulated activity of one (or more) would wipe out for consciousness the effect of the equivalent unstimulated activities of the others.[10]

The equation of response (3), represents the process of active stimulation while the equation of compensation (7), shows how the rate of repair varies with the sensitivity; (2) and (3) and (7) may be regarded as the fundamental and essential postulates of the theory. It is upon these postulates that the other equations, expressing the conditions of metabolic equilibrium, are directly dependent.

The equation of adaptation (10), shows that when a stimulus of intensity, i, is applied to a sensory cell the intensity of the response is not constant, but decreases as the time after the application of the stimulus increases. This decrease, however, itself decreases with the time, so that ultimately a point is reached at which the intensity of the response is practically constant. This condition is that of *equilibrium* with respect to the particular intensity of stimulus which is active. The relation which exists between a given intensity of stimulation, i, and the corresponding "equilibrium sensitivity," s, of the sensory cell is shown in equation (5), or, for compensating response in (8). It is clear that these equations, which are derived mathematically without further assumptions from (2) and (3), and, for compensating response (7), offer a rationale of the phenomena of sensory adaptation. The equation of recovery (11), on the other hand, which has a similar derivation, represents the process by which the cell returns to the non-adapted state.

The theory of metabolic equilibria above outlined is not presented as an exhaustive analysis of the relations involved in the metabolic process. It is offered simply as a crude generalization of the most fundamental principles underlying these equilibria, and hence can be regarded only as a first approximation to the truth. But, as such it must have a certain value.[11] Crudity of theory is more permissible in a field of measurement, such as psycho-physics, in which mean variations are large than in one where statistical procedure is a less essential part of the methodology.

The nature of further details of the metabolism of response which tend to make the situation more complex than we have represented it above will be discussed in the appropriate context below.

For convenience in our discussion let us assign names to the variables and constants employed in our mathematical reasoning, as follows: (1) $s =$ the sensitivity, (2) $\dot{s} =$ the resultant activity, (3) $a =$ the repair, (4) $k =$ the self-excitation, (5) $c_k =$ the constant of self-excitation, (6) $d =$ the response, (7) $c_d =$ the constant of response, (8) $c_r =$ the constant of repair, (9) $u =$ the compensation, (10) $i =$ "the stimulus" (stimulus intensity), (11) $q =$ the excitation, and (12) $t =$ the adaptation, or recovery, time.

The characteristic phenomenon of metabolic response is *adaptation. Adaptation may be rigidly defined as a specific but temporary reduction in the sensitivity of a sensory cell under the action of a stimulus of constant intensity.* It is the main thesis of this paper that the phenomenon of adaptation is the most important one which sensory psychology has to consider, and that a careful study of the laws of adaptation in the different departments of sense promises to be the most fruitful means of ascertaining the mechanism operative in these departments. Undoubtedly these mechanisms are not the same for any two senses, but certain general principles must hold for all of those senses which have the same general basis, such as, for example, the chemical. Both the resemblances and the differences between the laws of adaptation in the several sense departments should prove illuminating towards the determination of the exact mechanisms of the senses in question.

V. The Applications of the Theory

Let us now enquire to what extent, if at all, the theory of metabolic response as we have developed it is applicable to the several senses, as we now know them.

We should not rashly assume that all of the senses have the same mechanism, or even that they are all chemical. If adaptation occurs in a particular sense, and if the reduction of sensitivity which this implies depends in the general manner expressed by our equation of adaptation (10) upon the intensity of the stimulus, we shall have reason to suppose that the mechanism of this sense is chemical, and that the theory of metabolic equilibria will apply to it. Such adaptation, however, must bear the ear-marks of a peripheral process, and it must not be permanent in the absence of the stimulus. A sense in which there is no adaptation, or in which what adaptation does exist seems to be an adaptation of the attention, or to depend upon physical injury to the sense-organ, probably does not directly involve metabolic processes.

Fatigue may occur in senses which are not chemical, since all vital structures are, so to speak, tender, and when subjected to intensive or continued stress tend to break down, at least temporarily.[12] But the laws of such fatigue should be different from those characteristic of metabolic adaptation. Fatigue of this sort would not be specific with respect to the stimulus and it would probably not show equilibria. Moreover, it would be very much slower in recovery than chemical fatigue.

In how far adaptation of the sort which is typical of metabolic response may be simulated by processes which are not strictly chemical cannot be considered in detail in this paper. The type of adaptation in question, as illustrated, for example, in the case of vision, seems to depend upon the fact that something is being "used up" in the process of excitation. One naturally thinks of this "something" as a substance which is present in the beginning in a definite quantity, thus determining the special sensitivity of the sensory cell, and which is chemically decomposed by the action of the stimulus. Energy might also be stored and reduced in level during stimulation, but it seems hardly reasonable to suppose that the sense-cell needs to supplement the energy of the stimulus with energy of its own. Even if the sensitivity of the cell depends upon the amount of specific substance present within it this substance might conceivably be reduced in concentration asymptotically by some process other than that of chemical decomposition, such as for instance,—in the sense of touch —the migration of molecules or ions through a membrane under the influence of increased pressure. This last mentioned process would follow a law similar to our equation of adaptation.

In all investigation of this sort we are dealing in probabilities. However closely the phenomena which we observe may harmonize with deductions from our hypotheses, it is always at least remotely possible that another hypothesis would explain them as well, if not better.

With this digression let us return to the applications of our theory to the facts of sensory response.

The process of nervous conduction, Hering to the contrary notwithstanding,[13] is probably not a metabolic process. Although the changes which occur during the transmission of the nervous impulse are no doubt molecular they depend in all probability upon the displacement of ions rather than upon decomposition or synthesis.[14] If these latter processes occur in the course of propagation they are probably immediately reversed. Hence we need not look for adaptation in the conductional aspect of sensory response, but must seek it in the characteristic processes of the sense-organs.

A sense in which adaptation is certainly not a characteristic phenomenon is that of *sound*. Stimulation of the ear with a sound of constant intensity does not result in a decrease in the apparent intensity of the tone or noise, once the *tensor tympani* is adjusted, if the attention is unwavering.[15] This is what we should expect if Helmholtz's resonator theory of audition is correct, since, according to this theory the peripheral mechanism of this sense is purely physical. The theories advanced by Ewald and Meyer,[16] to explain auditory response are also stated in purely mechanical terms, and would not lead us to expect in audition any of the effects which would be characteristic of a chemical sense.

It seems likely that the *labyrinthine senses* are also mechanical, although it is difficult to say whether or not they show adaptation. The experiences of persons who are susceptible to sea-sickness seem, however, to point in the direction of their non-adaptiveness. Adaptation to the motions producing seasickness often occurs, but the length of time required for it to appear shows that it is not sensory.

If we turn to sensations derived from the skin, we find that although there are ample evidences of adaptation in the case of touch and temperature, the *sense of pain* appears to be non-adaptive.[17] Hence we should conclude that the last-mentioned sense is not chemical in its mechanism, and this conclusion is in harmony with the reasonable supposition that pain is due to the direct stimulation of the free nerve endings in the skin (and elsewhere).[18] According to this view, pain has no characteristic peripheral mechanism, and its non-adaptiveness is one with that of pure nervous tissue.

The *sense of touch* or pressure, on the other hand, is remarkably adaptive.[19] So far as extant experimental evidence goes to show, adaptation of touch follows the general laws contained in our theory of metabolic equilibria. That spontaneous excitation exists in the case of touch is probable, and is the basis of Fechner's theory of tactual space.[20] From the point of view of perception, however, the corresponding sensations *mean* the absence of pressure or of an outside stimulus. If for relatively weak stimuli the response of all touch cells finally reduces approximately to the level of the self-excitation, k, (equation (1)), we have an explanation for the fact that diverse pressure stimuli which are constantly effective ultimately become indistinguishable from one another, and from the absence of a stimulus. This principle seems to apply to pressures which do not exceed a certain maximum, such as the contact of the clothes with the body. For higher intensities of stimulation the sensation seems to remain above the neutral level for a long time, as we should expect from our theoretical discussion of compensation, above.

The fact that self-excitation exists in the sense of touch is indicated by the experience which we have upon the removal of a pressure stimulus to which we have become completely adapted, or for which the touch cells are in equilibrium. Since in accordance with the equation of equilibrium sensitivity (5), such an equilibrium means a reduction in the sensitivity of the cells in question the removal of the stimulus must be followed by a marked decline in the intensity of the sensation yielded by these cells, for equation (2) makes the self-excitation of the cells depend upon their sensitivity at the time. This fall of the local response below the normal autonomous equilibrium response provides the basis for our perception that "something is lacking."[21] The disappearance of this feeling as time goes on is accounted for in the equation of recovery (11), which demands the return of the sensitivity of such an adapted region to the normal.

The *muscular and articular senses* seem to be very closely similar, in their mechanism, to the sense of touch. Presumably, like the latter, they are chemical in nature, but nothing definite is known concerning their adaptation processes.

As we have already stated, Hering's theory of response metabolism has been specifically applied by its author to the *sense of temperature*.[22] According to Hering's view, heat favors katabolism in the temperature end-organs, while cold favors anabolism, the corresponding sensations being concomitant with an alteration of the equilibrium point from higher to lower, and from lower to higher, respectively. All equilibrium conditions yield the absence of temperature sensation.

It is clear that this scheme accounts very nicely for the balance which exists between the sensations of heat and cold, and especially for the peculiar facts of temperature adaptation. Everyone is familiar with the fact that a decrease in the sensitivity of the skin to heat means a corresponding increase in its sensitivity to cold, and *vice versa*. However, Hering's theory not only violates the canons of general physiology by joining a definite sensation (that of cold) with an anabolic process,[23] but it also explains the relations of the two temperature responses in terms which, although simple, are inconsistent with the demonstrated separate localization of " heat and cold spots."

If, then, we suppose that response for heat and cold occurs in different end cells, and that, moreover, they are both dependent upon the augmentation of certain katabolic processes, we have to account for the obvious antagonism which exists between them by means of an additional assumption. Heat and cold appear to have been correctly described by Hering as *antagonistic sensations*. As we shall see shortly, they are not the only sensations which belong to this class.

The nerve dissection experiment of Head and Rivers[24] has shown that there are at least two independent systems of temperature sensibility. The system comprised by the heat and cold " spots " gives no evidence of adaptiveness, and the reason for this is easy to see, in as much as the end-organs of this system are insensitive to temperatures lying between 26°C and 37°C. Since the curves of sensitivity of the heat and cold spots do not intersect, the temperature system which they make up can have no " physiological zero," and it is the shifting in position of such a physiological zero which constitutes ordinary temperature adaptation. The limits of sensitivity in this system are probably set by the precise nature of the chemical processes which characterize the end-organs. According to our view the heat and cold sensations yielded by the " spots " are antagonistic, like those of the " epicritic " system, but since only the latter show a physiological zero, it is only in the epicritic system that temperature adaptation can produce a qualitative shift in the sensibility.

Several of the experiments performed by Head and Rivers show that simultaneous stimulation of adjoining heat and cold spots result, under the appropriate conditions, in the inhibition of the heat sensations by the cold, or vice versa.[25] As the antagonism between these two sensations obviously cannot exist in the end-organs, as demanded by Hering's theory, it is necessary to assume that it has a central, or at least a *sub-receptoral* location. Let us assume, then, that the arrangement of the temperature nerves is such as to balance " heat " impulses against " cold," so that the intensity of one impulse is always subtracted from that of the other before any effect is produced in consciousness.[26]

Thus, if q_h is the excitation of the heat sense at any given time, and q_c is the simultaneous excitation of the cold sense, in a specified region of the skin, the effect upon consciousness will depend upon the arithmetic magnitude of their difference:

$$q_t = q_h - q_c, \tag{12}$$

the quality of the sensation being "heat" if the sign of q_t is positive, and "cold" if it is negative. If this hypothesis as to the relationship which exists between the heat and cold excitations be added to those of our theory of metabolic response it will be seen that this theory offers a satisfactory explanation for the phenomena of temperature adaptation so far as they are known at present.

The nature of temperature response is such that a stimulus is never absent. There is no physical zero for cold and the physical zero for heat never occurs in the environment of organisms. However, for purposes of exposition, we may define a "state of rest" of the temperature system in which

$$q_t = q_h - q_c = o. \tag{g}$$

If the temperature is now increased, q_h becomes larger and q_c smaller, in accordance with the equation of total activity (4), and hence a sensation of heat will be experienced. But the alteration which has occurred in the nature of the stimulus disturbs the equilibrium of both the heat and cold sensory-cells so that the excitation of the former gradually sinks in accordance with the equation of adaptation (10), while that of the latter gradually rises, as demanded by the equation of recovery (11). When equilibrium is re-established in both cells the equation: $q_h = q_c$, must again hold, so that $q_t = 0$. Although the temperature still remains at the higher point the sense of heat has vanished.[27]

In an entirely analogous manner it can be shown that if the temperature is lowered from this, or any other point for which equilibrium has been reached, a sensation of cold should be experienced until equilibrium again sets in, at which time the sensation will disappear.[28]

These deductions from the theory are obviously in general harmony with the facts of temperature adaptation. The deductions in question are based upon the assumption that the "repair," a, is a constant. However the fact that there is a limit (about 11°C.) below which complete adaptation to cold does not occur, and another (about 39°C.) above which complete adaptation to heat is reported to be unattainable, seems to show that, as in the case of touch the response is really "compensating," but that for the lower intensities of stimulation the compensation falls below the threshold. However, the experimental evidence is still too vague to permit surety upon this point.[29]

The sense of *smell* has long been regarded as a characteristically chemical sense, and there is no sense in which adaptation is more in evidence. There seems to be no moderately intense stimulus to which the olfactory cells will not become completely adapted in a relatively short time.

The fact that there is no manifest self-excitation in the case of olfaction seems at first sight to argue against the view that this sense is subject to our theory of the metabolism of response. However, the objection is not a fatal one, for two reasons: first, because there is evidence that antagonism exists between various phases of olfactory response, so that one species of self-excitation may blot out another; and, *second,* because the equilibrium excitation may lie below the threshold. If this latter supposition were made, an explanation would immediately be provided for the completeness of adaptation to all moderately strong stimuli, since for such stimuli the compensation may be insufficient to raise the equilibrium point above the absolute threshold. According to Nagel[30] complete adaptation does not occur in the case of very strong stimuli, a fact which is again corroborative of our theory of compensative response which demands that for higher intensities the excitations should never fall to a neutral level. Other aspects of olfactory adaptation are in harmony with our theory.

However, the writer holds the opinion that the mechanism of olfaction is a very special one and, although chemical, depends upon conditions which differ from those of metabolic equilibrium. He hopes to discuss this view in a later paper. But it is worth while to note that if olfactory response should ultimately turn out to be made up of component nervous activities,—as in the case of temperature sensation—we should probably have to deal with the relation of antagonism represented in equation (12) above, expressed, of course, in olfactory terms.

In the sense of *taste* adaptation is a well proven phenomenon In this sense, also, we find the relationship of antagonism between the separate qualities which are involved, although the antagonism is perhaps less well defined than in the temperature sense.[31] The four component sensations of "sweet," "sour,"

" bitter," and " salt " appear to be balanced against each other in such a way that the first is to a greater or less degree antagonistic to each of the others, the distinctness of the antagonism to " sweet " increasing in the order in which the latter are named.

It has appeared in the study of the temperature sense that adaptation to cold increases the sensitivity of the temperature system to heat, and *vice versa*. Similarly, it has been found by experiment that adaptation to salt raises the threshold for sweet. Not only this, but it appears that strong stimulation with salt may be followed by a sensation of sweet even in the absence of an adequate stimulus to such sensation. This latter is a negative after-image effect, and is immediately explained in terms of the theory of antagonistic sensations which we have outlined in connection with the sense of temperature and the possible application of which to olfaction we have just considered.

If self-excitations of the " sweet " and " salt " mechanisms exist these self-excitations must be equal and opposite in the state of rest, since in this state neither of the corresponding sensations is present although if it were not for the antagonism both sensations would be above the threshold. Adaptation to salt means a reduction of the sensitivity of the " salt " mechanism in accordance with the equation of equilibrium sensitivity (5). Consequently when the stimulus is removed the self-excitation of this mechanism must fall below that for the " sweet " mechanism, as demanded by the equations of self-excitation and of antagonism. This leaves a certain portion of the self-excited activity of the " sweet " mechanism unbalanced, the residue which is responsible for the negative after-image.[33]

Gustatory adaptation occurs slowly and for strong stimuli is never complete. Accordingly, gustatory response, if actually chemical in mechanism, must be to a high degree compensating.

The most careful of all studies of the phenomena of sensory adaptation have, of course, been made in the field of *visual* psycho-physiology, Hering, himself, having worked largely in this field. However, the phenomena of adaptation in vision do not appear to be such as completely to substantiate Hering's hypotheses concerning the exact mechanism of visual response.[34] These hypotheses suppose the visual system to contain three specific substances which in the state of rest have characteristic metabolic equilibria. Decrease in the concentrations of these three substances is accompanied by sensations of red, yellow, and white, respectively, while increase is paralleled by the opposite sensations, green, blue, and black, respectively. Now the facts of "color mixture" undoubtedly demand an antagonism between the excitations underlying red and green, and between those underlying yellow and blue, but observation does not support the view that there is antagonism between black and white. For these last mentioned qualities, on the contrary, there is fusion. An attempt has been made by G. E. Müller to remedy this weakness in the theory, but however successful, the attempt can hardly save the doctrine from the general objections outlined in the second section of this paper.[35]

To avoid these objections the present writer has proposed certain alternative hypotheses which form the basis of a theory similar in general structure to that of Hering but, he believes, somewhat less formalistic.[36] According to the author's view, the retinal cones contain four separate substances which are capable of being broken down by the action of light. The rates of decomposition of these four substances are the variables which determine the degree in which the corresponding sensation resembles red, yellow, green, and blue, respectively. However, a sub-receptoral mechanism of antagonism is further postulated, in accordance with which equivalent excitations of the red and green substances eradicate each other and are replaced in the same proportion by a new excitation giving the quality, white. The same relationship is supposed to hold for the yellow and blue excitations.

Now if we suppose that in vision, as in the other senses, self-excitation of the component responses exists, we are forced to assume that those for red and green are equal and opposite, and that those for yellow and blue are in a similar relationship. Under this supposition the so-called idio-retinal light can be referred to the white excitation which results from these two sets of balanced excitations. Such exact balance holds for autonomous equilibrium or for the normal rest condition of the retina.

If the eye is stimulated by a light which increases one of two antagonistic excitations more than it does the other the result will be a sensation corresponding in quality to the most intensive component of the response. As time goes on the intensity of this quality—say red—should fall in accordance with the equation of adaptation, until if the response were non-compensating the excitation corresponding to it would finally again equal that of its less stimulated antagonist—the "green" substance. When this point had been reached the sensation would be one of white or gray. If the stimulus were now removed the reduction in sensitivity of the "red" mechanism as compared with the "green," as demanded by the equation of equilibrium sensitivity, would cause its self-excitation to sink below that of the "green" substance, so that the quality, green, should predominate in the sensation which forms the negative after-image. Other things being equal, this sensation should be darker than the idio-retinal light, since the total activity is less than that which underlies that light.

The experimental study of visual adaptation proves it to follow the general equations of adaptation and recovery.[37] However, when we come to consider equilibrium conditions we find evidence that, at least for stimuli of fairly high intensity, adaptation does not proceed to a point at which the resultant sensation reduces to neutral gray. Consequently we are forced to assume that in the case of vision, as in most other senses, the response is compensating. Accordingly, the equations for allonomous equilibrium will resemble (8) and (9).

The curves of adaptation and recovery of vision have been studied both for the rods and for the cones of the retina, and have the general form demanded by our equations (10) and (11).[38]

In addition to the various sensations which we have considered above there is another psycho-physical process—or phase of activity—which shows in a most striking way the phenomenon of adaptation.[39] It may seem rather daring to assert that *pleasantness* and *unpleasantness* have a direct chemical basis, but that such is the case is certainly suggested by the existence of affective adaptation. The author has for some time been interested in the view that these fundamental and contrasted aspects of the psychical life—pleasure and displeasure—represent synthesis and analysis, respectively, in the chemical substance of specific cortical centers. He hopes to develop some of the implications of this not entirely novel view in another paper.

However, it is pertinent to note here that if this chemical theory of the physical basis of affection is correct the situation which it represents is not only one in which the fundamental equations of our general theory of chemical response should hold good, but is also one which very closely approximates that contemplated by the original katabolism-anabolism view of Hering. Hence, besides explaining the disappearance of the affective tone of a perception or sensation by means of our equations of adaptation, we can account for the reversal of the tone which generally follows the removal of the conditioning stimulus, in terms of the "assimilation or dissimilation material" which the physico-affective process has piled up. Pleasant experiences mean the accumulation of synthetized substance in the cortical cells. If the external causes acting upon these cells are taken away this overbalancing mass of anabolic material will favor katabolism, and hence the well-known "affective reaction." In a similar way we can account for the pleasure of "relief" which follows the removal of a painful stimulus or stimulus complex. This is the Hering doctrine, pure and simple.

Affective adaptation is typical and complete only in certain forms of experience, namely those in which the affective tone seems to depend upon novelty. Where the affection is determined by an instinct or some other basic mental "complex" the pleasantness or unpleasantness may be long continued and almost non-adaptive. We must suppose that in such cases there is an external repair process which (as represented in equation (7)) tends to maintain the typical metabolic function of the cortical cell by continually supplying it with the requisite chemical substances, whether synthetic or analytic. The beauty of meaningless music or the unpleasantness of some novel odor, are examples of the affective process at its simplest. With repetition this beauty, or disagreeableness, quickly disappears through adaptation. On the other hand, the pleasure of the erotic consciousness, and the unpleasantness of organic pain are affective processes which are undoubtedly supported by massive and complex psycho-physical mechanisms, of the nature of repair. This latter type of affective response, then, is compensative, like the majority of the sensations. On the whole affection tends to be compensative for low intensities and sensation for high intensities.

VI. Summary and Conclusion

If it is true that the characteristic activities of organic matter are at bottom chemical it is important for the psychophysiologist to investigate theoretically the relationships which exist between the processes of metabolism and those of sensation. Hering has done this with great generality and with significant results, but the development of physiological psychology since his views were proposed demands that the situation be reanalyzed and that his hypotheses be recast. Such is the purpose of the discussion which we are now summarizing.

Hering supposes that both of the balancing vital processes of anabolism and katabolism can be augmented directly by the action of a stimulus, and he believes that he finds an explanation of sensory response, adaptation, recovery, and especially of antagonism in the quantitative consequences of this supposition. These consequences constitute a theory of sensory equilibria, and according to Hering's doctrine sensation appears only in conjunction with changes in the position of these equilibria, shifts in opposite directions producing antagonistic sensations. However, the assumptions of this theory are inconsistent with the modern belief that the immediate process of stimulation is always katabolic, and many details in the consequences of the theory are at variance with fact.

Accordingly, an attempt is made to redevelop the metabolism of response along more modern lines. The postulates and the deductions from them are presented in mathematical form, and lead to a number of characteristic equations which are descriptive of the conditions and processes of simple chemical response. These equations are based upon the supposition that stimulation can directly influence katabolism only, and that the intensity of sensation depends upon that of the katabolic change in the sense-organ. However, a certain amount of katabolism is supposed to occur in the absence of a stimulus and this constitutes the "self-excitation" of the sensory system. On the other hand, the inherent repair process which necessarily exists as the ground of the katabolism may also be appreciably increased in the presence of a strong stimulus. When such increase occurs the response is described as "compensating."

The equations of response thus obtained are interpreted, and are then applied to the experimental facts of sensory psychology. Among these facts the most significant is asserted to be *adaptation*. As a result of our theoretical discussion we have been led to the view that adaptation may be used as a criterion of the presence of chemical mechanisms underlying sensation. The several senses are examined in some detail, with the outcome that those of touch, temperature,[40] motor activity, taste, and vision are found to be metabolic. The author expresses the opinion that the mechanism of olfaction is one peculiar to itself, although chemical, as commonly supposed. Audition and the labyrinthine senses appear to be purely mechanical and non-adaptive forms of response.

In the study of the senses of temperature, taste, and vision we find ourselves forced to explain the phenomenon of antagonism between certain qualitative different stimuli. This is accomplished in the present discussion by means of a mathematical assumption which differs from that of Hering, but which leads to the same general results.

It is suggested furthermore that the affective processes have a metabolic ground in the activities of the brain, and that affective adaptation is evidence for this view.

Hering's theory, then, although mistaken in the particular form of its postulates, and hence unfortunate in its attempted applications, contains a great deal of general truth, and should receive the correction and special elaboration which it has been the purpose of the present paper only to begin.

Appendix

1. This theory is given in its most general form in:
   Hering, E. Zur Theorie der Vorgänge in der lebendigen Substanz. *Lotos,* IX., 1888.
2. See:
   Hering, E. *Zur Lehre vom Lichtsinne,* Wien, 1878, pp. 74-85.
3. See, for example:
   Fick, A. Kritik der Hering'schen Theorie der Lichtemfindung. *Sitzungsbericht der phys.-med. Gesellschaft.* Würzburg, 1900.
4. The equation given is a "mass action" equation. Like (3) it depends upon the fact that the chances that a certain number of molecules of the sensitive substance will be decomposed in a given time are proportional to the total number present.
5. See reference 1, above: *"Sonderabdruck,"* p. 2.
6. See reference 1, above: *"Sonderabdruck,"* p. 4.
7. It is obvious that equation (7) specifies the conditions of an equilibrium between the cell and the lymph with respect to a transfer of the substance, S. Such an equilibrium exists when the cell is emitting as many molecules of S per second as it receives. However, this condition does not necessarily imply that the concentration in the cell is the same as that in the lymph at this time. As a matter of fact, even in the unstimulated cell this equilibrium will never be realized on account of the autonomous katabolism which is going on within the cell.
8. The equation of adaptation is derived as follows: Take $(c_k + c_d i) = b$. Then, by (4) and the definition of s,

   $$ds/dt = a - bs \quad \text{and} \quad -ds/dt = bs - a.$$

   Integrating the last expression; we get

   $$-\int \frac{ds}{bs - a} = \int dt + c \qquad \text{or}$$

   $$-\frac{1}{b} \log (bs - a) = t + c$$

But $c = o$, so that, transposing;

$log\ (bs - a) = - bt$, or, in exponential form:

$$bs - a = e^{-bt} = \frac{1}{e^{bt}}, \text{ whence:}$$

$$s = \frac{1}{b}\left(a + \frac{1}{e^{bt}}.\right)$$

On substituting this value of s in (6), we get:

$$q = a + \frac{1}{e^{bt}},$$

the equation of adaptation.

9. The equation of recovery was derived in the following manner: From equations (a) and (c) we have, when $i = o$:

$$ds/dt = a - c_k s$$

Transforming and integrating, we get:

$$\int \frac{ds}{a - c_k s} = \int dt + c \qquad \text{or}$$

$$-\frac{1}{c_k}\ log\ (a - c_k s) = t + c$$

Putting this into exponential form, we have if $c = o$:

$$a - c_k s = e^{-c_k t} \qquad \text{or}$$

$$s = \frac{1}{c_k}\left(a - \frac{1}{e^{c_k t}}\right)$$

Substituting this in the equation, $q = c_k s$, we obtain:

$$q = a - \frac{1}{e^{c_k t}}$$

It is obvious, however, that the form of this function must depend to a certain extent upon the value of s for which t is taken as zero. If we place $t = o$ in the equation as written we have: $q = a - 1$, from which:

$$s = \frac{a - 1}{c_k}$$

so that for the equation in the form given the recovery time must be measured from the instant at which s has the above value.

10. *vide infra.*
11. The writer desires to state explicitly that the present paper is regarded by him merely as a preliminary attempt towards an application of the method of mathematical hypothesis to the problems of sensory response. It is his opinion that this method would prove exceedingly useful in psycho-physics, if its nature and significance were clearly understood. Mathematical reasoning need not be limited to subject-matter which is capable of exact measurement. Its most general utility lies in the development and expression of *types of interdependence* between variable quantities. So long as we can *recognize the type* in the phenomena to which the mathematical results are applied, these results have significance. The reasoning presented in the present paper is in every sense schematic, and the hypotheses are tentative, although they are employed with that degree of confidence which is essential in any courageous attempt to discover the truth.
12. *Cf.*, for example:
    SCHÄFER, K. L. Der Gehörsinn, in Nagel's *Handbuch der Physiologie des Menschen,* III, 1905, p. 512.
13. See reference 1, "*Sonderabdruck,*" pp. 18 ff.
14. See, for example:
    LILLIE, R. S. The Relation of Stimulation and Conduction in Irritable Tissues to Changes in the Permeability of the Limiting Membranes. *American Journal of Physiology,* XXVIII, 1911, pp. 197-223.
15. For a definite denial of auditory fatigue, and a rather detailed discussion of the experimental evidence upon this point, see Schäfer, *loc. cit.,* pp. 506-512. A series of experiments to prove auditory fatigue, but with negative results, is described by:
    SEWALL, E. Zur Lehre von der Ermüdung des Gehörorgans. *Zeitschrift für Sinnesphysiologie,* XLII, 1907-1908, pp. 115-123.
    This investigator's method is the same as that of Urbantschitsch (1881) and Thompson (1881) who believed they had proven the existence of such fatigue.
16. MEYER, M. Theorie der Gerauschempfindungen. *Zeitschrift für Psychologie und Physiologie der Sinnesorgane,* XXXI, 1903, pp. 233-247.
17. See:
    TITCHENER, E. B. *A Text-book of Psychology,* 1910, p. 154.
18. See reference 17, p. 157.
19. There is some dispute as to whether touch adaptation is due to peripheral or to central causes, since it increases in the case of certain lesions in the cortex. However, it is a fairly safe policy to regard a process as peripheral until it has been proven to be central. On this, and related questions, see:
    THUNBERG, T. Physiologie der Druck-, Temperatur-, und Schmerzempfindungen; in Nagel's *Handbuch der Physiologie des Menschen,* III, 1905, pp. 647-735.
20. FECHNER, G. T. *Elemente der Psychophysik,* 2te unveränderte Auflage, II, 1889, pp. 311 ff.
21. See Thumberg, *loc cit.,* pp. 668-669.
22. HERING, E. Der Temperatursinn; in Hermann's *Handbuch der Physiologie,* III, 1879, (2), pp. 415-440.

23. It should be noticed that here the usual criticism of the Hering theory is not supported by chemical analogy. In general, decrease in temperature certainly does favor chemical synthesis. However, there is no evidence that effects of this sort in the living organism can directly produce sensation.
24. Head, H., and Rivers, W. H. R. A Human Experiment in Nerve Division. *Brain,* XXXI, 1908, pp. 323-451, esp. Chap. VII.
25. *Loc. cit.,* pp. 442-446.
26. Babák, E. Ueber die Temperaturempfindlichkeit der Amphibien. *Zeitschrift für Sinnesphysiologie,* XL, 1912, pp. 34-45, believes that he has evidence of a neural polarity of the heat and cold systems, in the nature of the reactions which heat and cold produce in the frog.
27. Voigt, A. Ueber die Beurteilung von Temperaturen unter dem Einfluss der Adaptation. *Zeitschrift für Psychologie und Physiologie der Sinnesorgane,* LVI, 1910, pp. 344-378, denies that temperature adaptation is complete, but it is to be doubted if the periods of adaptation which he employed in his experiments were adequate to prove his thesis.

    Rubin, E. Beobachtungen über Temperaturempfindungen. *Zeitschrift für Sinnesphysiologie,* XL, 1912, pp. 388-393, finds that temperature sensations exist only so long as the temperature of the skin is undergoing change. He proved by thermometric methods that such change may continue under constant conditions, for as long as an hour.
28. The investigations of Rubin, quoted in the preceding note, proved that the intensity of temperature sensation is greater when the rate of change of the temperature of the skin is greater. This is in harmony, of course, with Hering's theory, and it can be shown mathematically that it is also consonant with the hypotheses of the present paper, can in fact be deduced from them.
29. Some doubt must be felt as to whether the limits given are actually applicable to the temperature sensations themselves, since the extremes of temperature pass over psychologically into pain.
30. Nagel, W. *Handbuch der Physiologie des Menschen,* III, 1905, p. 613.
31. See Nagel, *op. cit.,* p. 643.
32. On these points, see Nagel, *op. cit.,* pp. 641-644.
33. Simultaneous as well as successive contrast occurs in the case of taste, but it is not our purpose to consider the basis of such contrast in the present paper, although it finds ready explanation in terms of the metabolic theory of sensory response. See the author's paper in the *American Journal of Physiology,* XXXII, 1913, pp. 34-36, for a statement of this explanation in the case of vision.
34. Hering's visual hypotheses are well stated in his *Zur Lehre vom Lichtsinne,* reference 2, above. For his discussion of visual fatigue and recovery, see:

    Hering, E. Ueber Ermüdung und Erholung des Sehorgans, Leipzig, 1891.

35. Müller, G. E. Zur Psychophysik der Gesichtsempfindungen. *Zeitschrift für Psychologie und Physiologie der Sinnesorgane,* XIV, 1897, pp. 1 and 161.

36. Troland, L. T. A Definite Physico-chemical Hypothesis to Explain Visual Response. *American Journal of Physiology,* XXXII, 1913, pp. 8-41.

37. For experimental data showing the nature of the curves, see:

Kries, J. von. Die Gesichtsempfindungen; in Nagel's *Handbuch der Physiologie des Menschen,* III, 1905, p. 216.

The curve of adaptation which is given has the right form if the relationship between the intensity of the stimulating light and the times of stimulation is taken into consideration. The curve of recovery is less satisfactory, but its lack of continuity throws doubt upon its validity.

38. In addition to the reference given in note 37, see, on adaptation in the cones:

Nagel, W. Helmholtz's *Physiologiche Optik,* 3te Auflage, II, pp. 282-283.

On adaptation in the rods (visual purple) see the same, pp. 264-278. Also:

Piper, H. Über Dunkeladaptation. *Zeitschrift für Psychologie und Physiologie der Sinnesorgane,* XXXI, 1903, pp. 161-215.

39. On affective adaptation see:

Titchener, E. B. A Text-book of Psychology, 1910, pp. 229-230, and Lectures on the Elementary Psychology of Feeling and Attention, 1908, pp. 65-69.

40. Evidence that the cutaneous senses are chemical has been found by:

Moore, T. V. The Influence of Temperature and the Electric Current on the Sensibility of the Skin. *Psychological Review,* XVII, 1910, pp. 347-380.

ON THE PSYCHOLOGY OF POETIC CONSTRUCTION

An Experimental Method

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What is the peculiar coloring of consciousness and what is the particular correlation of mental elements which characterize the processes of the poetic imagination? Precisely how can the poet's actual construction of a poem be described in psychological terms?

No positive answer to these questions is attempted in this paper. I shall at present endeavor only to outline tentatively an experimental method, an adequate application of which will, I believe, yield concrete information concerning the workings of the creative imagination during the process of poetic construction, and thus contribute to fill a gap in present psychological knowledge.

My first interest in the problem was stimulated by Professor Titchener, under whose direction the Cornell Psychological Seminary during the year 1907-1908 studied the problem of imagination. "Imagination is virgin territory, and awaits as it invites the pioneer:—"[1] and if this judgment of Titchener's represents fairly the status of the structural element, the image of imagination, it is doubly true of the higher complexes of the imaginative consciousness, especially of the processes of "inspiration" and poetic construction. I shall preface my outline of the proposed method by referring very briefly to several other methods used for the study of imagination, claiming no completeness of survey whatever and undertaking no criticism, but touching on their main points only in order to relate them to the method here proposed and so to indicate some positive merits (but also some scarcely avoidable disadvantages) which it seems to possess for the experimental study of the particular problem before us.

To a not inconsiderable part of educated humanity, this sort of inquiry has always appeared in the nature of poetic

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[1] E. B. Titchener, The Past Decade in Experimental Psychology; *American Journal of Psychology,* XXI, 1910, 417.

lèse majesté. For, while poetic construction has never ceased to interest mankind, the marvelous character of the poet's output has seemed to demand a mystical explanation. Mediocre humanity, regarding its mediocrity as normal, has seen in the prodigality of poetic genius a mark of super-normality, and has accordingly treated the poet as a god-inspired man, a madman divine. Witness the phrases 'inspiration,' 'divine fire,' '*poeta nascitur,*' and the poet's own traditional appeal to the muses. Plato writes in the *Ion,* "God takes away the minds of poets, and uses them as his ministers;" and Shakespeare:

> "The lunatic, the lover, and the poet
> Are of imagination all compact."

The bacchante is possessed by the deity; the poet is a priest of God; Handel sees Jehovah himself in writing the Halleluiah Chorus; Paganini's violin is devil-bewitched. This has been the popular explanation of the poet's work. Needless to say, on this basis we may be able to write poetry, but we can never explain the writing of it. Yet it must be confessed that even sober psychologists have often yielded to the lure of the mystical, and ended their chapter on imagination with a virtual confession of their inability to deal with it scientifically.

In the earlier psychology, of course, the imaginative faculty was discussed in a formal manner and was treated in a way calculated to fit it in the particular groove to which it was predestined by the logical demands of the philosopher's system: it was accordingly represented as thought involving internal intuition (Descartes), or as modification of sensation (Hume, Condillac), or else as the synthesis of reproduction operating in the dark chambers of the mind (Kant), and so forth.

The most cursory survey of the chapters on imagination in more recent psychological literature would reveal the absence of any agreement as to the nature of imagination, its relation to other mental processes, or its classification under some one general group of mental activity. Imagination is treated indifferently in connection with the representative, the affective, the volitional, or the higher thought-processes. James Mill styles it a mere train of ideas; Sully accentuates the choice of alternative combinations; James speaks of it as the recombination of previously experienced elements into new and original forms; Ladd stresses the teleological and lapses into mysticism; Calkins calls it the personal attitude of a self; Wundt describes it as a perceptual form of intellectual elaboration; Höffding treats it as the power of concrete ideation; Ribot discovers affective, volitional, motor elements. One could write a paragraph composed wholly of citations from standard psychological treatises, which would describe imagination as being everything that a mental process can well be, and would find in it, as the essential factor, sensation, feeling, will, association, reproduction, recombination! This state of affairs characterizing the treatment of imagination is only emphasized when we reach the paragraphs touching on the poet's imagination. The chapter on poetic construction is yet to be written in modern psychology, and written it can be, of course, only on the basis of experimental study.

The process of poetic construction may be studied on the basis of an experimental study of imagery. Indeed the scientific study of imagination during the last hundred years has been very largely a discussion of mental imagery. Bonald, Egger, Taine, Ribot, Arréat, Dugas, Philippe, Binet, Paulhan, Peillaube, Souriau, Galton, Bain, Meumann, Wallaschek, and many more have studied the types of mental imagery, visual, auditory, kinaesthetic, verbal, after-images, memory images, images of imagination. A very promising investigation of this sort is represented by the Cheves West-Perky experiments, conducted at the Cornell laboratory, the publication of which[2] has attracted considerable attention. The West-Perky experiments, at which I had the opportunity of serving as an observer, were calculated to lay hold on the image of imagination and, by comparing it with the image of memory and with perception, to furnish experimental knowledge of the structural elements of the imaginative consciousness. This sort of study is decidedly more definite and more relevant to our problem than the conventional treatment of imagery in general; but, in applying its conclusions to the problem of creative imagination, care should be exercised lest an incautious interpretation prejudice at the very start the success of a study like ours. For, it seems to me, one is scarcely warranted in proceeding from an experimentally elaborated and grounded difference (or else lack of essential disparity) between the image of imagination and the image of memory or perception, to the declaration of a corresponding difference or similarity between the processes of creative imagination and memory or perception, as the case may be. Would imagination cease to be a distinctive mental process if experimentation disclosed in it no structural elements other than those present in memory and perception? It may be possible that the poetic imagination is a unique elaboration of a wealth of material not different in structure from the stuff of memory and perception. The prosaic memory and the poetic imagination may be using the same alphabet of images, yet speak quite different languages. The problem of the poetic imagination, accordingly, requires an experimental knowledge, not only of *what* materials it uses, but also of *how* it uses them; how the poet combines usual conscious material in a most unusual way,—how, in the words of Browning's *Abt Vogler,* "such gift be allowed to man, That out of three sounds, he frame not a fourth sound but a star." To make it significant for the experimental study of our problem, therefore, the laboratory investigations of the image of imagination should be supplemented by a study of the process of poetic construction itself in such a way as to preserve its dynamic essence.

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[2] *Amer. Jour. of Psychol.*, XXI, 1910, 422-452.

This attitude toward our problem is, to be sure, by no means new, and I shall discuss briefly what seem to me to be the several typical avenues from which the study of poetic construction has been or might be approached. Thus, we may study the faint manifestations of what appears to be a poetic consciousness in mediocre minds. We may, for example, preface our study by attacking the problem from the angle of aesthetic appreciation: read a poem to a class of high school or college students and ask them to report the imaginative processes experienced. Then, passing beyond the mere enjoyment, we may ask a class of boys or girls or adults to write a poem, and then tell us how they did it. How far can experimenting of this sort carry us towards our desired goal: the solution of the problem of poetic construction? The verses, of course, will be doggerel; the introspections at their best will accordingly be introspections concerning the doggerel-writing process, not concerning the process of *poetic* construction. For we have no reason to suppose that the radical features distinguishing the output of a dunderhead from that of a Dante may not indicate a corresponding difference in the working of their imaginations. If we are to study the poetic consciousness directly, we must first of all find our poet:

> "Wer den Dichter will verstehen
> Muss in Dichters Lande gehen."

We must go to the poet's own country; we must study him on his native soil, in his own element. But how?

As one reads over the literature on creative imagination, especially the more recent French contributions, one is impressed by what may be called its gossipy character. In book after book one reads entertaining bits about poets, painters, musicians:—the vagaries of Théophile Gautier and the De Goncourts, the whims of Baudelaire and Balzac, the eccentricities of Daudet, the passions and sufferings of Chopin and De Musset, the luxuriance of Flaubert and Massenet, the cynicism of De Maupassant, etc., etc. I would not in any way depreciate the great value of this sort of work. The more we know about poets, the nearer we get to understanding the poetic consciousness; but surely the genius of Gautier did not exhaust itself in wearing outlandish waistcoats, nor is the literary work of Carlyle explainable quite in terms of his over-indulgence in ginger-bread. How, then, is the psychologist of the poetic imagination to become something more than a compiler of literary anecdotes? Is he to spy upon the poet unawares, dog his footsteps, jotting down every word in an effort to discover the trick of his inspired craft? Or is he to deposit him in a laboratory, and direct him to write poetry to the accompaniment of a smoked-drum motor, with intervals between stanzas for introspection? Or is he to extract his secret with a questionnaire?

Important work of a preliminary character has been done and can be furthered in the form of collecting, digesting, and presenting in systematic form the utterances of great and lesser poets on the subject of their art; the self-revealing snatches of verse or prose passages in which the literary genius, Whitman-like, "celebrates himself;" or else the more elaborate attempts at creative introspection,—writings like Edgar Allan Poe's *Philosophy of Composition,* purporting to give the history of *The Raven;* the "Studies of My Own Creation" by the poet Otto Ludwig, which Richard Müller-Freienfels has recorded in his *Psychologie der Kunst,* and others. A carefully worded questionnaire sent to contemporary poets of unquestioned genius, asking them for introspections concerning their methods of creative work, would be likely to yield revelations most valuable to the student of the problem. Or, going still further experimentally, one could select a genuine poet and simply Bozwellize him, study his imaginative consciousness directly, and secure the best introspective reports possible. Such studies have been repeatedly made in France, notably Binet and Passy's accounts of French dramatists, psychological reports of the literary work of Zola and Hervieu, and also Paulhan's record of the self-analysis of Roger Dumas, one of the younger French poets, containing an introspective history of the construction of two entire poems,—and most interesting reading it all makes too!

But all of these methods suffer from certain radical defects. Obviously we can not depend on a poet's self-revelational snatches of verse for the solution of our problem. One can borrow a couplet from Shakespeare or Shelley in order to illustrate vividly a conclusion already based on other grounds; but the psychology of imagination will not go far if it consists merely of poetical quotations. We can scarcely expect scientific exactness and accurate introspection from a poet who is expressing his inmost soul for other than laboratory purposes.

Far different is the case when the poet in a calm mood actually attempts to give an introspective account of his creative process. For that very type of mind which makes him a fine poet, makes him in most cases also a very bad psychological observer. Most of the poetic psychologizing along this line that I have read suffers from the irrepressible artistic temperament. The poet seems utterly unable to analyze himself with scientific objectivity. He sees things colored by his artistic preconceptions. He is either a mystic believer in the divine inspiration idea,—or else he is an Edgar Allan Poe type of a rationalist and tells you, step by step, just how he succeeded in deducing his great poem from his major premise. The student of imagination seeking light from the direct introspections of poets is thus obliged to steer his way between the Scylla of poetic mysticism and the Charybdis of irresponsible psychologizing. He is in the very palace of poetry, but the figures he meets all wear masks. Unless he can find his poet, he has little use for the introspections of mediocre minds; and, when he does find his poet, the latter turns out to be a most undependable psychological observer.

Laboratory experimenting with the actual process of poetic construction is confronted by radical difficulties, for the process is essentially spontaneous. Any conditions which interfere with the spontaneity of the creative imagination disturb the whole process. A poet can scarcely write genuine poetry and at the same time play laboratory observer. To be sure, a similar difficulty accompanies, in a measure, all psychological observing; but the very sense of partaking in an experiment, the very consciousness of having to report on what is going on, may result, in the poet's case, in nothing poetic going on.

I should once more disclaim any inclination toward drastic criticism. Each of the methods just cited has, up to a certain point, justified its application by yielding genuine information on the subject. My point is that, even in combination, they are approximative only and do not touch the heart of the problem. Experimentation with the image of imagination leaves open the problem whether the creative imagination may not perhaps be essentially an unusual way of using usual conscious material. Studies of the imaginative consciousness of mediocre minds are not assuredly relevant to the problem, for the difference between the mental operations of the poet and of the verse-monger may be as radical as is the difference between their respective products. Anecdotical discussions of the poet's characteristics, whims, and mannerisms are liable to lead to conclusions more interesting than significant. The final solution of our problem demands an experimental study of the poet himself; yet the psychological value of the self-revelational utterances of poets is at least partially vitiated by the poet's apparently irrepressible tendency to go to one of two extremes, and either ignore introspective clearness and accuracy in his zest for rich poetic expression,—or else lapse into the sort of psychologizing of which the artistic temperament, lacking the sense of scientific responsibility, seems notoriously capable. And the spontaneity of the process of poetic construction seems jeopardized when we ask the poet to turn laboratory observer while actually creating.

Are we, then, to give up the attempt at experimental investigation and rest satisfied with the roundabout ways of studying the process, satisfied too with the surmises and conjectures that have up to the present time formed so large a part of the conclusions concerning the poetic imagination? While thus seeking a promising avenue of experimental approach to the heart of my problem, I happened some years ago to be reading a volume of the Russian poet Lermontov. It contained two facsimile reproductions from the poet's manuscripts. One was a clean-copied page, and I took some pleasure in studying Lermontov's handwriting and reading his lines. The other facsimile was a first draft, very different in appearance from the clean copy: scratched up, scored and interlined, corrected to the point of illegibility. It was while doing my best to decipher Lermontov's script that the idea occurred to me, which I have since endeavored to develop and work out into a method for the experimental study of poetic construction.

Is not the problem of poetic construction a problem as to what goes on in the poet's consciousness while he is actually constructing? If some one had asked Lermontov how he wrote the poem in question, perhaps his answer would have been a wave of the hand, or a fanciful bit of irresponsible psychologizing. Yet this marked up copy in which word supplanted word, phrase phrase, and whole lines and stanzas were scratched up and crossed out to make room for their successors: is not this scarcely legible page a virtual laboratory record? If one could trace and establish the actual order in which pen-stroke followed pen-stroke, would not one be in a position essentially similar to that of the experimenter who reads on the smoked drum the zigzaggy record of his observer's reactions? Here are several false beginnings of a stanza, all suggesting different ideational and emotional contexts: the record of the progress from one to the other would be a record of the actual course of the poet's constructive process. Does the poet proceed with a clear view of his goal, and is his work one of selection from a rich store-house of image-combinations, as the typical British psychologists are inclined to believe? Or is the poet's work rather a progress from a vaguely felt mood to a mood more and more clearly experienced, a progress which is for the poet a voyage of discovery in which he really finds out what he is trying to say? Does the poem grow out of a catchy line or a jingle and is built around it; or is the pregnant phrase itself the child, the phraseologic elaboration of a central idea? Or, in case all of these processes are involved in the writing of genuine poetry, what mental factors are brought into play to emphasize one or another of these processes? All these questions, so puzzling at the present time and so likely to lead to mere theorizing, may find an adequate answer in the study of the experimental record represented by the poet's first draft. A poem's first draft is an objective record of a spontaneous process. In it the poet has, as it were, introspected while creating; step by step, the progress from mood to mood or from word to word or from image-combination to image-combination has found its inscribed record. Even the vicious scratches, the impatient or angry jabs with the pen, the idle lettering or sketching on the margin, the occasional smooth-running stanzas,—all indicate the progressive ideational-emotional coloring of the poet's consciousness. The correct interpretation of the first draft of a poem would thus be as correct an answer as it is experimentally possible to give to the question: what went on in the poet's imaginative consciousness while he was writing this poem?

The claim contained in the last sentence sounds bold, and yet it will be found modest enough. For this method has undeniable disadvantages. To begin with, first drafts of genuine poetry are not to be found as easily as I for one was sanguine enough to suppose at first. Poets have a curious reluctance to preserve these dirty-looking scraps of paper. A poem is written; it must be copied in its finished perfection, and then the luckless record of its natal hour is usually thrown into the fire or is torn to bits. Browning, for example, always destroyed his first drafts. It is only by chance that the illegible scraps of paper are saved. Still, long and patient search, especially in large libraries, will be sure to bring to light a sufficiently large number of the desired records, although it must be added that a poet's first drafts are likely to become the property of private collectors and are thus hard to trace. One such collector, however, has been good enough to print Shelley's Note Books, in three volumes, containing a good deal of valuable material.

In the second place, the almost illegible material must be deciphered, transcribed, and interpreted in the light of what is known of the character of the poet in question, his general mode of work, the uniformity or otherwise of his handwriting, etc. Literary erudition and psychological interpretation must thus go hand in hand in this sort of work.

Yet, even after this part of the work has been done successfully, the experimental conclusions reached will be valid only for the poem in question. Shelley's method of writing the *Lament* may be altogether different from Swinburne's or Byron's methods, different also from Shelley's own method in writing the *Ode to the West Wind.* Accordingly it will be necessary to decipher, study, and interpret as large a body of such materials as is possible. But such work will not be without its compensations, as it may throw light on neighboring fields of inquiry. Should a study of two poets disclose differences in their methods of construction corresponding to the differences in the character of their poetic productions, an experimental basis would be found for a good deal in aesthetics which at present rests only on theory. Of necessity, an investigation of this sort will have to be based on modern poetry alone, there being none of Virgil's or Dante's scratched up pages in existence. But that in itself will be no objection, if only a wide enough selection of first drafts from the great modern poets be available. If several genuine contemporary poets could somehow be induced to preserve their first drafts, without being aware that they are providing psychological material, much valuable information could be secured. Every successive interpretation will contribute to check up the experimental conclusions already reached and to establish them more firmly.

Even when all this is done, however, still one objection remains: what of the very most initial beginning of the poetic process: the so-called divine afflatus, the first breath of inspiration? That, I admit, cannot be reached by this method. This method is available from the first stroke of the pen: from that stroke on it is strictly experimental. But have we any reason whatever for believing that the continuity of the poet's creative process experiences a radical break precisely at the point when it begins to express itself on paper?

The limited time I have been able to devote to this problem has been spent largely in collecting, deciphering, and interpreting a few of the coveted first drafts. I have sedulously endeavored to avoid all speculation until an adequate supply of objectively established data could justify my proceeding to theorize. At present my supply is still meagre, but, while it does not warrant me in advancing any positive conclusions, it has abundantly vindicated my initial faith in this method as a means of studying, in genuine experimental fashion, the actual process of poetic construction. The present stage of my work gives me reason to hope that before very long I should be able to indicate, at least in broad outlines, the nature of the conclusions to which my inquiry has led me.

But I am becoming increasingly convinced that any generalizations reached by means of this method will be the more reliable the wider the field of poetic material which serves as their basis. This consideration has led me to overcome the natural reluctance of reporting on uncompleted work. My excuse for presenting this rough outline of efforts as yet unrewarded by positive results is that it may, perhaps, lead others, whose environment affords better opportunities for photographing or copying significant first drafts of genuine poetry, to interpret such poetic records with an aim similar to mine; and that, on the basis of several independent investigations of this sort, valid conclusions may be attained concerning the psychological description of the process of poetic construction.

THE EFFECT OF THE ATTITUDE OF THE SUBJECT UPON THE MEASURE OF SENSITIVITY

By Samuel W. Fernberger*

Some time ago, in an experimental study in lifted weights, the results of one subject showed that in a series of 1,200 judgments he had not judged a single pair as equal.[1] In such a series of results the thresholds in the directions of increase and decrease coincide; and hence the interval of uncertainty and the measure of sensitivity are zero. Woodworth[2] in a recent study remarks that theoretically this could happen and hence concludes that the interval of uncertainty is a poor measure of individual differences. As regards the first part of this statement, we agree. The interval of uncertainty should theoretically and does empirically reduce to zero. This is obvious inasmuch as the size of the interval of uncertainty is directly dependent upon the relative number of equality judgments which the subject may report in any given series. But the fact that this measure may reduce to zero is not a valid reason for believing that it is not a good test for individual differences. The thresholds that determine the interval of uncertainty are defined in terms of probabilities; and as such they are open to variation from a number of influences. Many of the objective factors that may cause such variation have been analysed from the total complex. For instance, we have standardized the psychophysical procedure so that the time and space errors may be eliminated. Woodworth believes that this reduction of the interval of uncertainty to zero, in an apparently normal series from the objective side, is due to a subjective factor. In our paper, mentioned above, we drew the same conclusion and stated that we believed that this was due to the subjective attitude of the subject inasmuch as he set for himself the *Aufgabe* of looking for a difference. But this introduction of a subjective factor does not invalidate the interval of uncertainty as a measure of individual differences. We must only note one other factor that must so far as possible be controlled in our experimental arrangement. It is of course true that since this is a subjective factor, it will be much more difficult to control than objective factors; but with trained subjects at least, introspections should reveal how nearly they have accepted the instructions of the experimenter. A measure of individual differences such as Woodworth apparently has in mind, would measure mere sensitiveness of the peripheral end-organ and not the sensitivity of the psychophysical organism with regard to our stimuli and our objective conditions.

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* From the Psychological Laboratory of Clark University. I am indebted to Professors J. W. Baird and F. M. Urban for many helpful suggestions in the preparation of this paper.

[1] S. W. Fernberger, On the elimination of the two extreme intensities of the comparison stimuli in the method of constant stimuli. *Psychol. Rev.*, XXI, 1914, pp. 335-355.

[2] R. S. Woodworth, Professor Cattell's Psychophysical Contributions. *Arch. of Psychol.*, No. 30, 1914. 66f.

Brown[3] has unwittingly shown most clearly the effect of the instructions upon the relative number of equality judgments. He strongly impressed upon his subject that she should be able to find a difference between each of his pairs of stimuli provided that she concentrated enough upon the judgment. Thus, by his instructions, he emphasised the attitude of looking for a difference; and the subject gave practically no equality judgments in a long series of liftings.

In order to test further our hypothesis that these variations were due to the attitude or *Einstellung* set up in subject by the *Aufgabe* or the acceptance of the instructions, we devised the following experiment in lifted weights. The usual series of seven pairs of weights were prepared, the standard weight of each pair being 100 grams, and the comparison weights 84, 88, 92, 96, 100, 104 and 108 grams. By means of a table provided with a revolving top we eliminated the space errors. The time errors were present in the first order but were kept constant by having the lifting controlled by the beats of a metronome.[4] The results were treated by the method of constant stimuli as developed by Urban. Fifty judgments upon each of our seven pairs of stimuli were recorded for each subject after he had been given a sufficiently long period of preliminary practice to mechanize the hand movements.

Fourteen subjects were employed, all of them graduate students in psychology at Clark University. None of these students knew this particular technique of lifting, before the experiment; and none of them was sufficiently well acquainted with the previous studies in psychophysics to be biased in the judgments. Furthermore, none of the subjects was informed of the problem which we were attacking. These subjects were divided into two groups of seven each. The stimuli were presented in a similar manner to the different subjects of these two groups and the conditions of the experiment remained as rigidly constant as possible, except for the instructions given the subjects at the beginning of the experiment.

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[3] Warner Brown, The judgment of difference, *Univ. of California Pub. in Psychol.*, I, No. 1, 1910. 1-71.

[4] For a detailed description of the type of stimuli employed, their arrangement upon the table, and the manner of lifting cf. S. W. Fernberger, On the relation of the methods of just perceptible differences and constant stimuli. *Psychol. Rev. Monographs,* XIV, No. 4 (Whole No. 61), 1913, 7-18.

The instructions for the technique of lifting and the like were similar for both groups. When we came in our discussion to describe the different categories of judgment to be employed, we here varied our instructions for the two groups. To Group I, we gave the normal instructions usually given to subjects for the method of constant stimuli: "You are to judge whether the second weight is heavier, equal or lighter than the first weight in each pair. There may be times when a doubtful judgment may occur, that is, when you are unable to decide whether the second weight in a given pair is lighter, equal or heavier than the first weight. In such a case announce your judgment as equal." Exactly similar instructions were given to Group II, except that we then added: "There has been more or less discussion in the literature of psychophysics as to the admission of the equality judgment. Some writers believe that it should be excluded. In my own experience, I find that the equality judgment does occur, both as actual subjective equality between the two stimuli of a pair, and also as doubtful judgments. I believe that you will find also that the equality judgment does occur in this series. Let me caution you, however, not to force any equality judgments; but do not hesitate to give such judgments if they do occur. Judge these stimuli honestly and to the best of your ability, but do not allow yourself to be prejudiced against giving them by any studies which you may have seen, which recommend that the equality judgment should be eliminated." We were exceedingly careful that the instructions should be given accurately to the different subjects in the two groups; and the subjects were requested not to discuss the experiment with any other students.

We believe that by means of these instructions we succeeded in giving different *Aufgaben* to our two groups of subjects, and as a result, different *Einstellungen* toward the problem. The usual instructions, such as we employed in the case of Group I, seem to give the subject the attitude of looking for a difference rather than of using equally his three categories of judgment. This attitude may have given rise to the entire question in the literature of eliminating the equality judgment, because the subject uses the three categories differently. Working under the influence of his *Aufgabe,* he first searches for a difference; and when he fails in this, he then turns toward the equality judgment. On the other hand, by emphasising the equality judgment, as we did in our instructions to Group II, we believe that we made the three categories of more nearly equal value.

The effect of these different *Aufgaben* and the resultant attitudes toward the experiment, become evident when we examine the size of the intervals of uncertainty obtained for the two groups. These values for Groups I and II appear in Tables I and II respectively. The tables are similarly constructed. The subjects are indicated in the first columns; in the second columns are given the values of the interval of uncertainty $(S_2 - S_1)$ for each of the subjects. In the third columns are recorded the value of the measure of sensitivity $\left(\frac{S_2 - S_1}{2}\right)$. Finally in the last columns are to be found the values assumed by the point of subjective equality $\left(\frac{c_1 + c_2}{h_1 + h_2}\right)$ for the different subjects. In the bottom rows of the tables will be found the averages of the values in each of the columns.

When we compare the values of the interval of uncertainty for the different subjects, we find that those obtained from Group II,—with whom the equality category was emphasised in the instructions,—are very much larger than those from Group I,— who worked under normal instructions. Indeed, the largest individual value for this interval for Group I is smaller than the smallest value for Group II. On the other hand, the values obtained for Group I,—who worked under the normal instructions,—are quite similar to those obtained from other studies where a similar technique was employed.[5] Thus the intervals for Group II are considerably above the normal. The difference between the averages for the two groups is exceedingly large, being 3.504 grams. This is nearly 63 per cent. of the average value for Group I. Hence the interval of uncertainty for Group II is considerably more than half as large again as that for the first group. Obviously the same relations hold for the measure of sensitivity since it is merely one-half of the interval of uncertainty. On the other hand, the averages of the points of subjective equality for the two groups are exceedingly similar, being 95.77 grams for Group I and 96.22 grams for Group II, a difference of only 0.45 grams. Such a variation is well within the limits of what one might expect in such an investigation. This means that although the relative number of equality judgments for the two groups varied considerably, still the relative number of heavier and lighter judgments showed little variation one with the other. Or in other words, in Group I the judgments which were not given as equal were divided between the heavier and lighter categories with approximately the same relative frequency as these two categories occurred for Group II.

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[5] cf. F. M. Urban, *The Application of Statistical Methods to the Problems of Psychophysics,* Phila., 1908.
S. W. Fernberger, *ibid.*

It would seem that the exceedingly large variations in the size of the interval of uncertainty indicate that Group I had a higher sensitivity than Group II. But we do not believe that this is the case. One can not measure sensitivity in absolute terms; one can only say that a given sensitivity has been found to exist *under certain given experimental conditions.* Inasmuch as we are dealing with a total psychophysical organism and not merely with a sense-organ, it is, after all, not surprising that the attitude of the subject should have a profound effect upon this sensitivity. The above results indicate that this is true empirically. The investigation of such subjective factors is one of the main problems of the psychophysical methods. The similarity of the results in former studies indicates that for most subjects, similar instructions will arouse similar attitudes toward the experiment. Now and then a subject will set for himself a different *Aufgabe* than the one which the experimenter tries to have him accept. Usually this is because the subject desires to 'do well' and believes that the giving of equality judgments is an indication that he can not discriminate so accurately. Such a reaction is to be found in the subject described in our former study. But in such a case, the experimenter is able, quite readily, to ascertain this fact from the exceedingly small size of the interval of uncertainty itself. The experimenter should be able to ascertain the attitude and *Aufgabe* of the trained subject from the introspective reports. But for the untrained subject—that is, if the methods be applied for diagnostic or anthropometric purposes—the methods must be used with greater caution and with a realization of the fact that here is a subjective factor which may cause exceedingly large variations in the measure of the sensitivity of the subjects.

TABLE I

Subjects	Interval of Uncertainty	Measure of Sensitivity	Point of Subjective Equality
I	6.28	3.14	94.72
II	5.80	2.90	95.22
III	6.74	3.37	98.23
IV	4.91	2.45	92.98
V	7.08	3.54	96.96
VI	3.64	1.82	95.54
VII	4.50	2.25	96.75
Average.......	5.56	2.78	95.77

TABLE II

Subjects	Interval of Uncertainty	Measure of Sensitivity	Point of Subjective Equality
I	7.32	3.66	97.86
II	7.72	3.86	97.02
III	10.70	5.35	95.00
IV	8.79	4.40	95.87
V	9.22	4.61	96.42
VI	9.36	4.68	94.38
VII	10.37	5.18	96.96
Average.......	9.07	4.53	96.22

THE ASSOCIATION METHOD IN ITS RELATION TO THE COMPLEX AND COMPLEX INDICATORS

By Samuel C. Kohs, A. M.,
Clark University

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Footnote.—The writer wishes to express his earnest appreciation of the valuable aid, suggestions, inspiration so frequently, willingly, gladly offered by his leader and guide, G. Stanley Hall, before, during, and even after this article was in its formative stages.

A. Introduction

A large number of present-day tendencies in psychology seem to be directed toward individual differentiation and subjective analysis. The farther we advance into these realms of investigation, the more are we overpowered by the burdens of our ignorance. Nevertheless, the contributions made to psychology in the last decade or two, show conclusively that we are successfully wedging our way into the dark, hidden recesses of our "Denkfabrik." Our senses are as yet too dull, too undeveloped, too inaccurate to appreciate the finer, inner workings of our psyche. That some day the science will be in a position to explain a large number if not all of the mental mechanisms is the confident belief of many. At present, we must be content with objective results, verified and supplemented by introspection.

B. Some Psychological Concepts

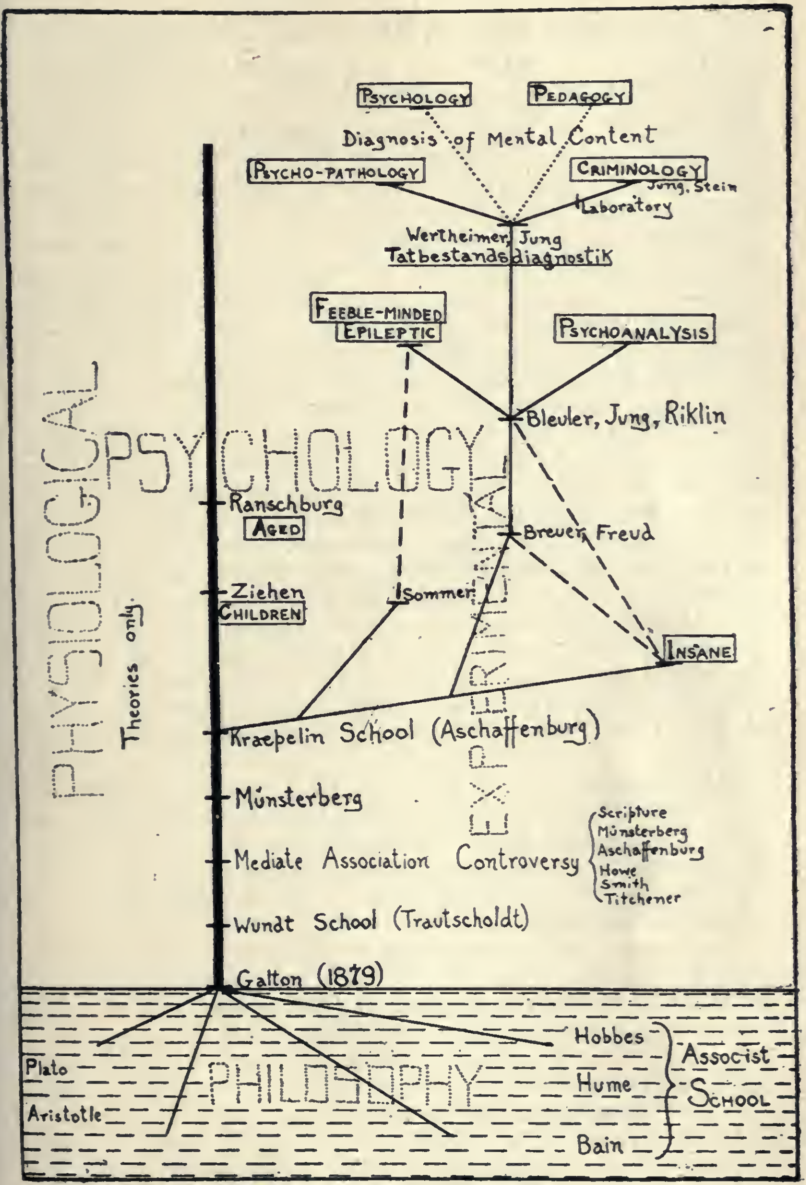
The process of association is, from the synthetic point of view, the keystone of our mental life. It would be folly to maintain at this date, with Hobbes, Hume, Bain and the rest, that the phenomenon of association is the *all* of consciousness. We must admit, however, that it is one of the basic factors making for normal mental activity. Perception, thought, action cease when the association process is interfered with. The laws of memory, of habit, of thought, are but a restatement of the laws of association.

Besides frequency, vividness, recency, primacy, intensity, and emotional congruity, there are a large number of factors which tend to influence the relations between ideas. (It is these *relations*, by the way, which we have in mind when we speak of "association.") And every attempt to arbitrarily classify these connections into different categories, outer, inner, egocentric, mediate and so on, has failed in many respects. This condition has largely been due to one or more of these reasons: (1) the tendency of classifications to be artificial; (2) their tendency to be superficial; (3) they are not based on introspection, and as a result they are objective instead of being subjective; (4) the separate categories are not sufficiently exclusive; (5) the experimenter may not correctly interpret the response; (6) the presence of mental imagery aroused by the stimulus is but poorly expressed by a single-word response; (7) our associative procedure is probably too complicated to bear a minute analysis, as yet.

It is not surprising, then, to find Cordes criticizing the early experimenters in this field for making such a simple process of the associative mechanism. He attempts an improvement by dividing it into two divisions, naming one the "A"-phenomenon, which includes the reception of the stimulus and the character and state of consciousness at the time, and the other, the "B"-phenomenon, which includes the response and the mental state at that time. This analysis is but a mere beginning. We have not advanced very far beyond it since. It is interesting to note that Jung and Riklin, mindful of Cordes' conclusions, designate their experiments as "verbal reactions to stimuli."

According to Hobbes, the natural state of one's ideas is uncontrolled, desires and wishes directing the train of thought. Where will and desire are more or less destroyed, as in dementia, the flow of ideas remains uncontrolled. It is recognized now, however, that ideas come to consciousness not only independently of our will, but oftentimes even against it. We see the extreme form of this in melancholia and in the obsession psychoses. For Wundt, this directing is done by apperception. He differentiates between perception and apperception as follows: In perception the idea is present in the field or "Blickfeld" of consciousness; in apperception, the idea is in the center or "Blickpunkt" of consciousness. Ziehen maintains that Wundt drives this theory of apperception a little too far, making the process superior even to that of association. The assumption that apperception is a sort of special selective faculty located somewhere in the frontal lobes of the brain, makes one justly wonder whether Wundt is not here being unconsciously influenced by the old faculty psychology. Sutherland emphasizes the fact that as yet we know too little of apperception to undertake a study of its influence on one's train of ideas.

The neurologic mechanism of association is still a matter of speculation. But whether it be conceived as neural habit, least resistance, "drainage," or what not, these phenomena *are* apparent: the process of association remains constant in its manifestations, it obeys certain laws, it can be studied experimentally, and the results may be utilized for educational and psychotherapeutic purposes. The nervous system receives and stores impressions. It consequently presents the paradox of being unchanged in what it has already acquired, and of constantly changing through what it is continually receiving. We must also recognize that the unconscious is the great storehouse of impressions.



A TREE OF DEVELOPMENT
to illustrate the progress of association experiments

C. Significance of Association Phenomena

Keeping this in mind, let us ask what is the significance of association phenomena. To Bleuler, "The process of association epitomizes the complete psyche, the past as well as the future with all of its experiences and aspirations. It is an index to all mental phenomena, which we only need to decipher in order to obtain a complete knowledge of man."

Semon has given to science the "mnemic" theory, the main thesis of which had long before been held by such men as Claude Bernard, Haekel, Orr, Cope, Nägeli, Hertwig, Hering, Samuel Butler, and which is now being so ably supported by Hering, Semon and Francis Darwin, among others. It postulates, first, the possibility of stimuli leaving traces or engrams in an organism; and secondly, a dormant condition of engramatic characters in both soma and germ-plasm, which become active as the individual develops. According to this theory the growth of the chick from the egg is merely an act of memory long imprinted upon the animal mechanism. With the conjugation of the germ cells, memory begins its marvelous process of regular, unchangeable activity. Each completed act serves as a stimulus for the next one. What appears a rather simple structure, is literally charged with vast complexes of memories. The greater the charge, of course, the higher up is the organism in the phyletic series. Ribot even goes so far as to say that "memory is essentially a biological phenomenon and only accidentally a psychological one."

Much as one wishes to adhere to Weismannian principles, it is very difficult to conceive that progress leaves no transmissable trace or "engram" upon the individual. The modification, no doubt, is most microscopic, most minute. But without postulating its existence, how can we explain many of the facts of heredity?

Man's progress up the evolutionary ladder has not been a smooth or regular one. To reach his present state of development, he has been compelled to pass through severe, ofttimes cruel, sharp struggles in order to obtain food, warmth, shelter, protection, for the purpose of preserving the life and vigor of both individual and species. The slow convulsions of nature forced him into an ever-constant adaptation to climate and environment. He was driven into perpetual experimentation, with its attendant sore trials, harshly-reacting errors, as well as triumphant successes and happy achievements. The joys and fears, the pleasures and pains of the intense effort upward must have left their traces somewhere in the organism.

Like the cells of the developing chick, we are often impelled to acts, to thoughts, by a something in us which is inexplicable, undefinable. Schopenhauer, in his essay "On Virtue," says the following: "The lover is deluded in thinking that he aims at his own happiness. The will to live is forcing him, for nature's own purposes, to aim at a certain typical beauty, which has its end in the perfection of the offspring. The more perfectly two individuals are adapted to each other, the stronger will be their mutual passion. Nature is striving for a better realization of the type, and to attain its ends must implant a certain illusion in the individual. That which is good only for the species appears to him as good for himself. This illusion is instinct. The individual is but a helpless tool carrying out blindly the designs of nature." Le Bon, in his "Psychology of the Crowd," says, "behind the avowed causes of our acts there undoubtedly lie secret causes that we do not avow, but behind these secret causes there are many others more secret still, which we ourselves ignore. The greater part of our actions are the result of hidden motives which escape our observation." Swoboda and Fliess have demonstrated the theory of periodicity in human activity, a phenomenon quite independent of volition.

Just as the human frame is an "old curiosity shop" full of unnecessary antiques, so also is the human psyche a vast storehouse for the stirring, epoch-making events of phylogenesis. Since refined surgical and histological methods reveal a mass of rudimentary and vestigeal organs, may we not also expect that refined psychological procedure will reveal as much, if not much more, on the mental side? Already psychoanalysis has made an excellent beginning.

Leaving these purely theoretical and hypothetical considerations aside, we will now enter the problem proper, with a short review of the experiments in association for purposes of better orientation.

D. Historical Survey

The phenomenon of association had been observed by Plato and Aristotle. They classified the relations between ideas into logical categories. The study and discussion of this whole problem was continued in the realms of philosophy culminating in the extreme views of the associationist school. The experimental work began with Galton in 1879. His method was simple and crude. He, nevertheless, obtained very valuable results. In 1880, Wundt took the problem into his laboratory and in 1883, Trautscholdt published an extended article on the subject. In 1885 followed Wahle. In 1889, Münsterberg published his "Beiträge," giving an account of association in adults. Three years later began the controversy on mediate association, Scripture, Aschaffenburg, Münsterberg and Wundt being the participants; later Cordes, Howe, W. G. Smith and Titchener were added.

It is natural to expect that the first movements should be concerned with the *mechanism* of association, the process being considered detached from the individual. Rittershaus, however, is unrelenting in his criticism. He distinctly emphasizes the fact that it is altogether impossible for us to possess a purely abstract psychology of association before we possess a concrete, individualistic one. He, like many other critics, takes these experimenters too harshly to task failing to realize that the whole problem was passing through a normal, wholesome evolution.

Beginning with Kraepelin in 1896, we notice a deviation from past methods, first as regards the aims of the experiments, and secondly, as regards classification. Kraepelin was the first to outline an extensive program for the investigation of neuropathic and psychopathic conditions. He studied the effects of practice and drugs on association. Of his students, Bethmann investigated the relation of mental work to changes in association,—Aschaffenburg, the effects of fatigue and exhaustion,—Weygandt, the effects of hunger. Aschaffenburg made some radical changes in the hard and fast, iron-bound classification of the Wundtians. He readjusted it to permit the inclusion of marked deviations due to some abnormality.

In France, Ribot was the first to emphasize the significance of individual differences and to differentiate types; Bourdon was mostly concerned with classification, and Dugas did work similar to that of Ribot. In America, Cattell, Calkins and Jastrow were the chief workers in this field. Eighteen hundred and ninety-eight saw the appearance of Ziehen's masterful contribution to the association of ideas in children. His predecessors in this field were many, but Ziehen was the first to make a thorough, scientific study of the subject. He also attacked the logical method of classification because it neglected content and was only interested in form. Unfortunately, it was hard for Ziehen to break away wholly from these preconceptions, and the results obtained from adults influenced his methods, as well as the interpretation of his data, too greatly.

At about this time appeared Ranschburg's article on association in the aged. In 1899 appeared Sommer's "Lehrbuch." By 1906, the Zürich School was already well organized and it began its epoch-making series of contributions headed principally by Jung and Riklin. From these studies dates the work which will be more intensively discussed later, and which makes apparent the enormous significance of the phenomena of associated ideas. Jung and Riklin opened the doors of the subconscious. Immediately the complex was seen in its true significance. At present, this knowledge is being utilized in three directions, first, to determine the relation of the complex to aetiology in mental disorders; second, to determine its relation to mental content; third, to determine its relation to impulses, ambition, ideals, character.

For bibliographies, the following may be consulted since they cover a large part of the literature (see bibliography):

Claparède. L'association des idées. 1903.

Ley and Menzerath. L'étude expérimentale de l'association des idées. 1911.

Rittershaus. Die Komplexforschung. 1910.

Lipmann. Die Spuren interessebetonter Erlebnisse und ihre Symptome. 1911.

E. Constellation and Complex

I. *Definition*

Coming to our problem more specifically, let us define our terms. The literature is very vague in the use of the words "constellation" and "complex." All writers, with but few exceptions, have a tendency to handle them very loosely, carelessly and ambiguously. For that reason, a definition which is accurate and at the same time in accord with common usage is practically impossible. I will make an attempt, however.

Ziehen was the first to use "constellation" in its present significance. To him "constellation" was an associative grouping of various ideas around a central idea. Thus,—assuming *a* to be a central idea, there may be associated with it in consciousness, *b, c. d. e.* etc. At one moment the recall of *a* may recall *b;* at another, *c* may be recalled; and so on. Jung and Riklin modify this definition by adding that the experience creating a constellation of ideas must have been individual, subjective, have occurred within recent time and must possess a distinct feeling-tone. As it is used to-day, "constellation" means an associative arrangement of specific mental data. This mental data may consist of sensations, perceptions, ideas, impulses, desires, feelings, and so on. Any of these, and all of these in various combinations, are the units which compose the constellation. The mental data must not necessarily be pure ideas, as is held by some. In analyzing constellations, it will be found that each is composed of a different mosaic of mental elements, differing in character, significance, intensity and meaning. Also, since sensations, perceptions, memories, always possess affective accompaniments, all constellations, which are only hierarchies of these mental elements, possess some *feeling* tone. In many instances, it is so slight as to remain unnoticed, and in others it is so intense as to force the constellation to become transformed into the complex. The difference between a constellation and a complex is a difference of intensity in affective-tone. Education, environment, home influences, age, sex, race, religion, certain hereditary predispositions, are among the most important factors determining the type, number, and trend of one's constellations.

As has already been indicated, the complex is merely a constellation possessing a more intense affective-toning. Some writers maintain that this feeling element must be one of pain. With this position I disagree, since pain is only a small portion of the enormous field of affection. The constellation becomes a complex when the affective content changes from feeling to emotion. It must be particularly emphasized that the existence of complexes in consciousness or in the unconscious, is neither pathogenic nor pathognomonic. The phenomenon is perfectly normal. We all possess complexes. We are aided by some, we have to contend with others, still others are sublimated and we are better off as a result. It is the neurotic who suffers.[1] Any complex coming into conflict with ethical or aesthetic principles may, after a struggle for dominance, be repressed (verdrängt) into the unconscious where it continues its efforts to reach the " Blickpunkt," the center, of consciousness.[2] With neurotics these complexes increase, unify and exist apart from the primary personality. In this manner, one or more secondary entities or personalities may be created which live a life apart from the primary one. The complex thus obtains expression. With normal people the complex often obtains externalization through symbolism, displacement, conversion, transference, and through the channels of literature, art, humor, dreams. For further elucidation of these, and other phenomena, the reader is referred to the psychanalytic literature.[3] We are all, in a slight manner, however, examples of split-off personalties. This fact becomes clearer as we note a man's intellectual advance with age, the process of specialization becoming more and more apparent. All of us are a bundle of characters, of selves, each very different from the other, and each in varying stages of development. The individual at a baseball game is apparently totally unlike the one delivering a lecture on metaphysics; he is totally unlike the one leading a "gang" of boys into the open country; very different from the one standing judge over the seriousness of his moral transgressions; and totally unlike the one confessing deep and earnest affection for a pretty representative of the weaker sex. Yet all of the metamorphoses of self are due to changes of the predominating mass of complexes and constellations. These diverse personalities are normal manifestations, all being moulded into an harmonious unit. Synthesis is the keynote of mental hygiene. Where complexes have been repressed, and are split off or dissociated, there one is certain to find definite psychopathologic conditions.

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[1] And in this respect, the recent psychoanalyses of psychopaths and criminals, on the one hand, and geniuses, artists, poets, on the other, reveal that the operation of the complex in both types is very much alike. Consequently, our future treatment of the social deviate both before and after committing an offense must take this fact into consideration.

[2] Freud gives some interesting explanations in this connection with regard to forgetting generally, as well as infantile amnesia and cover-memories ("Deckerinnerungen"). The censor, an active, mental principle similar to Wundt's apperception, selects and rejects ideas as they come to consciousness.

The following are some conclusions relative to the emotional content of the complex by experimenters in this field: Birnbaum believes that the affective-toning possessed by a complex changes with time. According to Burr and Geissler, most of the normal, every-day complexes, and there are many of them, are not marked by very strong feelings. Jung and Riklin observed that during distraction the number of complexes discoverable in a given subject was greatly reduced. They also found, and to this Sutherland takes exception, that complexes do not necessarily have to be apparent to the subject, for the experimenter to be certain of their existence. They usually remain unnoticed until the association data are properly evaluated. According to Jung and Riklin's findings, confirmed by Rittershaus, most of the complexes are of an erotic, libidinous nature, thus emphasizing their great rôle in human behavior. Gallus stresses the fact that sometimes complexes are uncovered where one would least expect to find them, and at other times, where one is most certain of their presence, none exist.

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[3] And here, perhaps, a word might be said concerning Freudianism. Many of the Freudian postulates cannot as yet be accepted in their entirety by the critical mind, as explanations of some existing conditions. None the less, there is a good deal which is valuable in the analyses, but one must be discriminating. The attempt to reduce so much of human activity and defectiveness to a sexual basis (in its broadest sense), is bound to fail. It is generally known and recognized that the predominating forces of life are not *all* exclusively libidinous.

II. *The Unconscious*

The complex and the unconscious are of great significance for normal psychology as well as for psychopathology. Investigations of hysteria, dementia præcox, paranoia, have already revealed a mass of data concerning the mechanism of the complex. All neuroses, according to the Freudians, are based on definite complexes. Normal psychology is now ready to take the path marked out by these pioneer workers in the field of mental disorders. The phenomena of forgetting, moods, character, likes and dislikes, ambitions, mental and physical ability, habits, the development of special aptitudes, all stand in close and intimate relationship to the complex and the constellation.

Probably the most important deduction one can make from "Komplexlehre," is that: *The moment a complex is created, it becomes a potent dynamic unit.*

F. The Association Methods

We now come to a consideration of the various procedures employed in detecting the existence of constellations and complexes. My thesis will include only the so-called free-association methods. In reality there is no "free" association. The relations between ideas are not accidental, but predetermined, and various factors influencing the appearance of an "Einstellung" will also influence the train of thought.

The aim of the association methods herein described is mainly to indicate the existence of complexes, or the possession of knowledge of a certain type (crime, or "Tatbestand" [constellation of facts]). Their application can be made to cover a much wider field, and may perhaps some day aid in the solution of the problem of higher mental tests for the Binet scale. The fundamental principles underlying the methods are as follows:

(1) An experience leaves some sort of trace in the psyche. Ideas are linked up which were not previously related, or the bonds which did previously exist are greatly strengthened.

(2) Relations between ideas can be proven to exist by the association method.

(3) By means of a properly arranged experiment upon two individuals, one possessing a specific constellation and the other not, one may determine fairly safely (using the response, qualitative, quantitative and physiological, as a criterion) which of the two possesses the specific constellation.[4]

I THE ASSOCIATION SERIES

(1) *The Series Proper*

(a) *Number of Words*

Although there is a difference of opinion as to the number of words composing the series, it is generally held that 100 is the "happy" medium. According to Ley and Menzerath, the Jung series of 100 or 200 words is entirely too long. They favor one ranging from twenty to fifty words. Aschaffenburg points out that where 200 words are used as stimuli, the conditions under which the first twenty-five are reacted to, cannot be the same as those for the last twenty-five. Comparisons, consequently, may give entirely false results. At the other extreme is Kramer who maintains that the 100-word series is too small. It must be kept in mind, however, that fatigue may easily set in, and that the subject's attention may begin to be removed from the main purpose of the experiment. Lipmann states that 100 words should be the smallest number employed, and actually favors a longer series, since length finally destroys dissimulation. The more a person has reacted, the more will his mode of reaction become mechanized and the more independent will it become of volition. Repression and inhibition being reduced to a minimum, the complex or constellation can consequently be more easily reached. He believes that the larger the series, up to a certain point, the better the results. But a good deal depends on the purpose of the experiment, the mental type of the subject, the speed of the experimenter, general conditions and circumstances, for all of which allowances must be made. As to the time, Ziehen and Sommer never permitted their experiments to run over twenty minutes. It was usually between ten and fifteen.

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[4] The distinction between an association test and a psychanalysis is this,—the association test establishes or confirms the existence of relations between ideas. It stops there. The psychanalytic method goes farther, and attempts to explain or interpret these relations by a closer, more intensive questioning, and its final aim is catharsis, "chimney-sweeping." The psychanalytic concept, however, is broadening out to mean more than what I have indicated.

Woodworth and Wells state that it requires from ten to twenty-five minutes to give the K. R. series of 100 words. Jung sets no limit as to time, but if fatigue is observed at the end of the first hundred words, the second series of 100 are reserved for another day. This has never been necessary with educated subjects. In conclusion, Löffler suggests that the association series ought to be different for every individual.

(b) *Character and Arrangement of the Words Chosen.*

The association series ought to be composed of relevant and irrelevant stimulus words, by relevant words meaning those related to a complex, and by irrelevant, words unrelated to it. Only to the first type is the subject expected to react suspiciously. The relevant or significant words are assumed to be irrelevant or insignificant to controls. Of course, in actuality, this is never entirely possible. Words of double meaning ought also to be included. Lipmann summarizes the necessary requirements of relevant stimuli by saying that they must be such as not to touch complexes possessed generally, they must not be unusual, difficult to understand, exceptional, they must not be easily distinguishable from the irrelevant either in the number of syllables or in grammatical form. Such procedure as that of Van der Hoeven, two irrelevant words being placed after each relevant one, is not scientifically advisable. There ought to be no regular array of significant and insignificant stimuli. Lipmann adds that the series should be so arranged that the subject will be surprised when confronted by a complex stimulus. He, consequently, advises the irrelevancy of the first ten words in order to mechanize the mode of reaction. Löffler and Lipmann also suggest arranging the words so that before a significant stimulus is reached an "Einstellung" of such a character will have been created that a complex-reaction will be greatly facilitated. Lipmann advises the following order of irrelevant and relevant words in the series: (r = relevant; i = irrelevant; s r = strong relevant)—ten i—four r—ten i—six r—eight i—one s r—six i—six r—fifteen i—one s r—ten i—eight r—eight i—seven r. (Total, sixty-seven i and thirty-three r.) Rittershaus is very much opposed to this arbitrary schematization, stating that no experimenter is able to formulate a series of relevant and irrelevant stimuli since no one but the subject can determine their character, and then only by means of his responses. In his experiment Schnitzler used twenty critical words which were as gross as possible (forceps, blood, etc.). According to other investigators, this greatly militated against the accuracy of his results. Thus

Löffler and Rittershaus maintain that gross, coarse stimuli ought not be utilized in searching out a painful spot. For, just as the physician is careful of his movements, tender in his touch during a diagnosis, so also must be the explorations of the psychologist, who is seeking for complexes in the innermost reaches of the psyche. With regard to critical stimuli, no set list can be given which might be employed by all. It must be remembered that conditions, subjects and experimenters vary greatly. However, Jung found that these words, among others, usually gave long reaction times: needle, hope, strange, false, heart, pyramid, strike, threaten, remember, ripe, to woo, fern, hair, nauseate, dream, paper, book, harm, softly, caress, family, consciousness, freedom, faith, violence, wonder. According to Wells, using the Kent-Rosanoff series (twenty-five cases), the ten words giving the shortest reaction times were: bitter, ocean, white, dark, yellow, swift, thirsty, sour, stove, scissors; and the ten words of longest reaction time were: square, command, anger, religion, moon, slow, health, justice, stomach, wish. Words to which some of the men (physicians) failed to respond were: intend, never, interest; and women (nurses):—disgrace, betray, expect, information, cravat, magnet, diagram.

(c) *Trial Series.*

It is advisable to give each subject a short explanation of the method to be pursued with short, simple, definite directions for reacting. A trial series of insignificant material consisting of ten to twenty words ought to be employed mainly for the purpose of making the directions and information concrete, and permitting the subject to become better oriented to the problem. By means of this trial series, the experimenter can obtain a better insight into the mental type of his subject. Kramer and Stern used a trial series of 100 words, but this length is neither necessary nor profitable.

(2) *Presentation of the Stimulus*

(a) *Mode of Presentation.*

There are two forms in which the stimuli may be presented. First, in auditory fashion, either by the experimenter, pronouncing them,[5] or as Lipmann suggests, by means of a phonograph. (The latter, Rittershaus calls " scientific fool's play.") The stimuli may also be presented visually by using the tachistoscope. But Rittershaus advises against the use of this extra apparatus, since it only complicates the procedure and may disturb the results. Besides,—some subjects cannot read very easily, some may have some apperceptive difficulty, others may feel compelled to translate the seen word into auditory, motor, or voco-motor terms in order to grasp its meaning. In the last case especially, the effort may result in an audible pronouncing of the stimulus which, by some careless experimenter, might be interpreted as a mechanical repetition and diagnostic of a complex. The tachistoscope, nevertheless, has the advantage of being independent of any personal influence of the experimenter, the stimuli remain constant, which factor also favors the use of the phonograph for auditory stimuli, and pictures, colors, etc., as well as words, may be presented. Lipmann indicates the value of the tachistoscope for the phenomenon of " Verlesen," of misreading,—e. g., a person guilty of murder, instead of seeing the stimulus "Mond"—will see it as "Mord." The same holds true of the phonograph (Verhören), the word Olch is heard as Dolch (dagger) by the murderer, as Strolch (tramp) by the tramp, and as Molch (salamander) by the innocent. Cordes maintains that visual stimuli are stronger and better to work with than those of an auditory type, and that ideas come more spontaneously when the word is seen than when it is heard. As we have remarked above, this latter depends greatly upon the individual. He goes on to state that we are more accustomed to *seeing* isolated words than to *hearing* them, and that the *amount* of impression remains constant with visual stimuli, and cannot remain constant with stimuli of the auditory type. Ziehen, in studying the associations of children in the fields of color, space, number and time, utilized various methods of presentation.

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[5] As yet, we know but little of the mental processes which are brought into activity in the subject by the manner and tone in which the stimulus is given. Cordes earlier, and Levy-Suhl later, both emphasized this point.

In the older experiments, where words were presented visually, the subject was seated in a dark chamber, entirely soundproof. Pictures, symbols, colors, colored letters or words, single letters, nonsense-syllables, were presented by means of an exposure apparatus. This procedure is not much in vogue now, although Wundt prefers it to others.

On the whole, the best procedure is to have the experimenter pronounce the words himself, aiming, at the same time, to keep conditions as constant as possible. Lipmann suggests that the tempo ought not be too slow. The next stimulus should be given as soon after the response as possible. Thus the constellation is kept intact and no opportunity is given the subject to prepare a response. Aschaffenburg obtained, on the average, 200 associations in forty minutes or five associations per minute. In other words, in about every twelve seconds the whole process of presenting the stimulus, recording the response and holding a protocol was complete. Cordes states that this time is too short. Other authors give no data. Although it seems advisable to standardize the time, yet subjects and experimenters present such wide variations in rate of activity, that it would be almost impossible to strictly adhere to any set time without injuring the experiment. If any additional apparatus is used, such as a galvanometer, a pneumograph, automatograph, etc., more time must be allowed. This has its disadvantages, of course. Lipmann also states that the rendition of complex-stimuli ought not be especially emphasized in tone, in pause, or accent, no changes ought be noticeable in gesture or facial expression of the experimenter.

(b) *Instructions*

The method pursued in the association experiments is as follows: The experimenter gives the stimulus word and the subject is asked to respond with the first idea that occurs to him.[6] This "Aufgabe" may be put in various forms—"answer as quickly as possible with the first word that comes to your mind."—or "with the first thing this word makes you think of."—(In German,—"zu antworten mit das erste Wort das klar zum Bewusstsein kommt"—or "mit dem nächst einfallenden Wort zu antworten.") That the response will be but a small portion of a complicated association-complex is self-evident. Nevertheless, valuable results may be obtained, despite this handicap. Wertheimer and Klein have shown that only a slight stimulus is necessary to bring to light a whole crime-complex. Schnitzler, on the other hand, altogether denies the possibility of arousing a single association of a definite kind by means of a single stimulus word.

The procedure in which idea *a* calls up *b*, *m* calls up *n*, *g* calls up *h*, is called the reaction method. This differs from the *serial* method in which when *a* is given, *b* is associated.

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[6] In reality, this procedure is only a simplification of the one usually adhered to in obtaining information, namely, questions are asked and the subject or patient is required to answer them to the best of his knowledge and ability. On the basis of this material, its amount and character, one is often able to pass a fairly safe judgment.

Then follows *c* in association with *b,* then *d* in association with *c,* and so on (a➳b➳c➳d [C. S. Meyer]). Only the first type is being considered in this paper.

Sutherland found that a slight variation in stating the directions produced no observable difference in the response. Ziehen, working with children, sometimes repeated directions in order to hurry them on. This fact was always noted on the record sheet. He found no difference in results when the stimulus was given in a soft or a loud tone of voice. It would be much more advisable to keep *all* conditions as constant as possible *all* the time. In many experiments such as those of Kramer and Stern and of Rittershaus, it was found advisable to have one person give the association-series and note the response, while the other recorded the time.

(3) *Response*

To quote Wells, "In spite of all instructions to the subject about the 'very *first* word that comes into your mind,' etc., and the best coöperativeness, it is naïve to suppose that we get this response from the ordinary subject save in a comparatively small number of cases." Aschaffenburg also makes the observation that after the stimulus word is given, a large number of ideas may suddenly come into the conscious field of the subject. Usually, one is more dominant than the rest and thus finds expression. Trautscholdt and Wertheimer indicate that the reaction to the stimulus may come to consciousness first in terms of sensory, motor or kinaesthetic imagery, also as moods and feelings. From introspective accounts, the latter found that sometimes the stimulus word, as such, comes into consciousness. Other times it was a visual image of an article in the room, of which the complex-arousing word was a part. All imagery differed in intensity, clearness, and duration. Sometimes a peculiar awareness or attitude (Bewusstseinslage") was present, there was a consciousness of *seeking* something, of trying to remember, *e. g.,* (to quote from the introspections) "That is a word (content) which I have but shortly seen," "that is somewhat critical," "that is betraying," "I ought not to express this." Wertheimer also observed that the appearance of a reaction word was always present with a strong impulse to express it. The following are some of the reports: "The complex-reaction burst out suddenly without permitting me sufficient time to judge whether I wanted to say the word or not." "The desire not to express a betraying word was very strong, nevertheless the content was of such great strength that it forced itself through these barriers." "I could do nothing else; I knew perfectly well that I ought not to say the word, but I could do nothing else, the pressure was too great." "I wanted to substitute another word, but I uttered the word which I did not want to." "It took me unawares, I could not stop it." "The complex-word was present with such power, that it suppressed any emerging irrelevant word." "I could think of nothing else." In the cases where reaction-words were successfully repressed, a peculiar condition, called a "mental vacuum" was produced. These are some of the reports: "There was a complete emptying of conscious material." "It feels as if everything had disappeared from consciousness, an absolute lack of ideas." Very often this condition of mental vacuum was accompanied by a distinct feeling-tone, described as "uncomfortable," "painful," "a feeling of tension, suspense," "a feeling of perplexity." In some cases it even took the form of "a feeling of fear, no word will come into consciousness, time is quickly passing, perhaps a complex-word will appear." This condition ceased when a word was finally spoken or when an active "search" was instituted for an appropriate response. In this connection, it might be mentioned that Schnitzler observed a number of cases where an irrelevant response followed quickly after the complex-stimulus was given. In such cases, the real significance of the stimulus word became apparent to the subject only after the irrelevant reaction had already been expressed.

The recorder is expected to note everything possible—the response, the reaction time, the behavior, all movements and expressions. The significance of this data will be discussed later under complex-symptoms.

(4) *Miscellaneous*

(a) *Sex and Personality of the Experimenter*

Both Pfenninger and Morawitz indicate that the psychosexual "Einstellung" of a subject must have some effect on the response and reaction time. This "Einstellung" naturally, varies with the sex of the experimenter. Sutherland also maintains that the type of the investigator (personality, voice, sex, social position) and the type of the person examined (sex, social position) both affect the results before, during and even after the experiment is complete, when an attempt is made to interpret the data. Aptekmann (Zürich laboratory) may be said to be the first to make a direct study of this problem. Her procedure, however, is open to grave criticism. Her subjects were six educated men and six educated women. Jung and Aptekmann each served as experimenter and both their results were compared. She concludes as follows: (1) The *male* experimenter produced greater affective changes in the subject. (2) Where the experimenter and subject were of the *same* sex, more *social* complexes appeared; where they were of the *opposite* sex, more *erotic* complexes appeared. More data are sorely needed.

(b) *Factors Inflencing Associative Relations*

(1) Age.—Ziehen found that children respond with *particular* ideas, whereas about eighty per cent. of adult responses are by means of *general* ideas,—again confirming the long-established fact that children deal with particulars. The reaction time with children is longer than with adults. Children give about two per cent. verbal or word associations. With adults this percentage is much increased. Ziehen and Meumann found that children between six and thirteen who gave a larger proportion of particular ideas, were the brighter. Ranschburg found that adults gave 11.8 per cent. more inner associations than younger people. Jung and Riklin found the same to be true in their investigations. In other words, the older a person, the greater the probability that words will be associated by content, by inner significance, and not by mere sound, or any other objective relation. The first type of response (inner association) results in a longer reaction time. This relative difference does not change with age.

(2) Sex.—Jung states that the method is as yet not sufficiently developed to show the finer sex differences. Still he and others did obtain some very interesting data. Rittershaus has indicated that the larger portion of complexes belong to the sex sphere. The percentage is greater in women than in men. Wells observed that of the rather long reactions, the men gave eighteen, the women seventy-two. Haggerty and Kempf have pointed out that a large part of sex differences were due to attempts at suppression and substitution. Women show a shorter reaction time compared with men when there is *no* inhibition, and a longer time than men when inhibition is active. These results agree with Pfenninger's findings. Wells noticed that some women show marked deviations from the average of their sex. Sometimes, consequently, a careless statistical treatment of data may greatly falsify the facts. The statement, so often made by Jung, that women give longer reaction times, does not hold strictly. An examination of his

data makes Wells conclude that there are two types of women, (1) those whose responses conform to the average of their sex, these subjects giving a shorter reaction time than men, (2) those whose responses show great deviations. The two groups ought to be considered separately, especially when making generalizations in the matter of reaction time.

Jung and Riklin found that men give a larger proportion of egocentric reactions. Educated men and educated women differ only slightly in their responses. Uneducated men show a much lower type of reaction than uneducated women, this difference being due very largely to man's meagreness of affectivity and subjectivity. This, of course, does not hold for educated men. From these results, one is naturally led to believe that educated men possess more feministic characteristics than uneducated men. Which is perhaps additional confirmation to those who believe that woman is higher in evolutional development than man.

Kramer and Stern observed that, in revealing the existence of a complex through content, only *one* reaction was needed in the case of men. With women, on the other hand, the diagnosis is not as simple and requires a number of responses before judgment can be passed. H. Gross explains this phenomenon on the basis of woman's almost instinctive tendency to secrecy. A woman has more to hide than a man, *e. g.*, menstruation, pregnancy, her interest in her husband, etc. She must be more careful in her speech than man is. With the development of society, its customs and taboos, she has finally acquired a natural tendency to keep from revealing her innermost thoughts. Perhaps this is so, and woman's reputation for talkativeness may only be an Adlerian "compensation" for her "minderwertigkeit" in these other directions. Other experimental data, such as that of Jung, does not seem to bear this out.

A quantitative, but not qualitative, examination of attention by Jung and Riklin indicated that uneducated women are superior to educated men in this respect, due largely to the fact that the situation to the uneducated is new and strange; the experiment being more like a mental test, attention is consequently concentrated. The educated individual approaches the problem as an easy, simple one, and attention is not intensely concentrated.

(3) Race.—Aschaffenburg merely mentions the necessity of further research in this direction, but as yet I have found no work dealing directly with this phase of the problem.

(4) Family.—Jung and Riklin noticed resemblances in the reactions of members of the same family. Emma Fürst confirmed their findings.

Jung and Riklin, for example, cite this case:

Age	Inner	Outer	"Klang" or Sound	Egocentric
Mother over 50............	75%	19%	0%	40%
Older daughter about 38....	56%	39%	1%	15%
Younger daughter about 35	35%	58%	5.5%	7%

This data, however, is liable to be a little misleading. One would suppose that when the younger daughter attains her mother's age, her reactions would be the same as those of the parent. This cannot be the case. For example, comparing the two sisters who differ in age by about three years, it is very, very questionable whether the younger sister will in three years give 56% of inner associations instead of 35%, an increase of 21%—or give only 1% of sound associations instead of 5.5%.

Besides concluding that relatives show a marked agreement in reaction type, Emma Fürst also adds that children tend to resemble the reaction type of the mother more than that of the father. There is a smaller difference between father and son, than between father and daughter,—and a smaller difference between mother and daughter, than between mother and son.

(5) Intelligence.—Rittershaus indicates that subjects coming from a lower level of intelligence are not accustomed to think in terms of single words or ideas, but in sentences. Believing that the experiment is a sort of intelligence test, they tend to say as much as they can in response to the stimulus.

The following is taken from Jung's data:

The reaction time of educated men—averages 1.3 seconds;
The reaction time of uneducated men—1.6 seconds.
The reaction time of educated women—averages 1.7 seconds;
The reaction time of uneducated women—2.2 seconds.
Average for the educated, 1.5—for uneducated, 1.9.

Jung's results also show, to one's surprise, that the uneducated give more inner associations in the following proportions,—43:36%, and less outer associations,—53:59%. Because of this greater proportion of the inner type, the uneducated give a longer reaction time.

Sommer and others have found that the feeble-minded, some of the insane and the epileptic show a striking poverty of ideas and tend to give stereotyped responses. Among others, Sommer cites this case:

Male: dement and epileptic.
Out of forty-six reactions—
table was the response to 13 of the stimulus words.
bone " " " " 10 " " " "
house " " " " 3 " " " "
child " " " " 4 " " " "
and dog " " " " 3 " " " "
Some of the long reactions were—
Hand—bone—18 4-5 seconds.
Room—table—27 1-5 seconds.
Lung—lung—22 seconds.

Moravcsik states that those possessing strong mental balance answer with single words instead of with more than one word or with a sentence. Schnitzler warns that many uneducated and ignorant individuals very often may give reactions which appear suspicious without there being any actual basis for it.

(6) Occupation.—Aschaffenburg experimented on two groups of physicians, the first consisting of five, and the second of four, subjects. His results with the first group were as follows:

39 out of 100 reactions were common to two physicians.
16 out of 100 reactions were common to three.
4 out of 100 reactions were common to four.
2 out of 100 reactions were common to all five.

In the second group:

23 out of 100 reactions were common to two.
14 out of 100 reactions were common to three.
0 out of 100 reactions were common to all.

Jung and Riklin give stronger evidence in the following two cases. One is a teacher of natural history. In the first half of the experiment, 26% of his reactions were related to his school work. In the second half, the percentage was twenty-one. In the first half, the name of the school is repeated four times, and a word relating to a certain event which occurred there, twice. The second subject is a chemist. Nearly all of his constellations are related either to his laboratory work or to his love affair. Rittershaus found many complexes in his subjects which were related to their occupations or habits.

(7) Emotion.—Aschaffenburg observed that anger, similar to overwork, insufficient sleep, exhaustion, tends to increase the number of associations by assonance and rhyme. According to Ziehen, emotions greatly influence reaction time,—feelings of pleasure facilitate associations; feelings of pain inhibit

them. This is excellently exemplified in the mental disorders,—mania on the one hand and melancholia on the other. Jung found that the stronger the affective tone possessed by a stimulus word, the greater will be the attention which the subject concentrates upon it,—and in consequence, the number of inner associations will be increased. This process then is just the reverse of distraction.

It is unfortunate that aside from these general considerations, there is nothing else, bearing particularly on this subject, in the literature. Birnbaum in 1911 made a study of the influence of affective factors on association, yet one cannot but be convinced that had Birnbaum utilized all that his predecessors had contributed with regard to method and interpretation, much more valuable and significant results would have been obtained.

(8) Beliefs and Ideals.—Jung and Riklin found that these sometimes stood out very clearly because of their constant repetition in reply to stimulus words.

(9) Fatigue.[7]—Aschaffenburg's results may be summarized as follows (Sommer):

(a) Under the influence of exhaustion, the relationship between stimulus word and reaction word becomes looser and looser, and may be easily broken depending upon the intensity of the original attraction between the two ideas. In this condition, the language relationships predominate, the stimulus word is not perceived by content but by sound. (b) Exhaustion tends to increase the reaction time. This difference is only slight. (c) As exhaustion proceeds, reactions by assonance, rhyme, word-completing and supplementing, and speech reminiscence, increase more and more. (d) Exhaustion brings with it an easier impulse to react. No attempt is present to inhibit the reaction.

Jung and Sommer question whether all of the above phenomena are due always and wholly to exhaustion. Fatigue may serve as a reducer of inhibitions, in which case complexes may more easily be brought to light.

(10) Drugs.—(Alcohol, tea, coffee, etc.) In general, their effect is similar to that of exhaustion. It would be a most interesting problem to determine the effect of internal secretions upon the association process.

(11) Familiarity with Language.—Trautscholdt had observed that unfamiliarity with the language increased the proportion of outer associations.

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[7] Kraepelin differentiates between weariness and fatigue, the former serving merely as a danger signal for the latter.

(12) The Grammatical Form of the Stimulus.—The uneducated, Jung found, possess a greater tendency to react in the same grammatical form, noun to noun, verb to verb, and adjective to adjective. For the educated, the average correspondence was fifty-one per cent.—for uneducated it was fifty-nine per cent. Verb and adjective stimulus words reduce the number of noun reactions to about half. Verbs and adjectives give rise to a larger number of inner associations. Ziehen observed that verbs and adjectives produce longer reaction times than nouns. The most difficult stimuli are, for educated men, concrete nouns, and for uneducated men abstract nouns.

(13) Attention.—This factor will be considered later in discussing the distraction experiments, and the inability to concentrate as a complex-sign.

(14) Practice.—Wells summarizes the practice effects as follows: (a) They tend to decrease the association time. " Practice shortens the central tendencies of the times, it does not bring down their lower limit very much,"—that is, practice does not affect the most rapid reactions. (b) They tend to remove certain inhibitions, the vocabulary of the subject becoming more available. (c) They tend to decrease the emotive value of the experiment. As a result, this element reduces the value of the association method for complex-diagnosis.

II THE ASSOCIATION SERIES WITH REPRODUCTION

(1) "*Wiederholungsversuch*" (Recall of Response) (Jung)

This procedure was first used by Jung and was based on the phenomenon of repression (Verdrängung). Since individuals attempt to repress and forget those thoughts and experiences which are out of harmony with prevailing moral and aesthetic concepts, consequently those stimulus words related to any disagreeable, unpleasant or offensive acts or ideas, will not elicit the same response when the identical experimental series is again given.

Jung's method was as follows: He first constructed the regular series of 100 words to which the subject responded. The reaction, both qualitative and quantitative, was noted. After a moderate interval (the following day), the subject was again told to react to the same series, and as far as possible, with exactly the same words. This time only the verbal response was noted. (In Pfenninger's reproduction experiments, the same series was given at eight weekly intervals.) A comparative estimation of results proved that the reactions to words related to a complex were not the same in both cases.

Lipmann found it difficult to estimate failures of reproduction when a synonym or an approximately similar word was given as the second response. In such instances, the number of other coincident complex-indicators ought to be used as a criterion in deciding. Jung, Pappenheim and Peters have shown that false reproduction and long reaction times occur together most remarkably. Schnitzler, however, observed a negative correlation. Langfeld, working on the problem of negative instruction, the subject avoiding one type of response in preference to another, proved that under these conditions the failure of reproduction went as high as 26%.

(2) "*Erinnerungsversuch*" (Recall of Stimulus) (Wertheimer, Lipmann).

First, the association series is given. The response and reaction time are noted. The "Wiederholungsversuch" described in the last section, may or may not follow, it being optional with the examiner. After a reasonable interval, the subject is asked to recall, in any order, as many of the stimulus-words as possible. These are recorded. Lipmann finds that the larger percentage of words remembered will be complex-toned, and that the first words recalled may be of great significance in diagnosis. The repetitions during recall are important,—these also should be recorded. During reproduction, if words appear which were not present in the regular series, a close examination may reveal the relation to the complex of both the incorrectly reported word and the forgotten stimulus. Lipmann remarks that this "Erinnerungs" procedure may result in the subject's gathering together the scattered complex-stimuli in such an order, that the sequence of ideas may almost amount to a voluntary confession.

III The "Aussage" or Reproduction Method

IV The "Aussage"-Association Method

V The Combination Method

In these experiments, the examiner assumes that the subject has had a specific, vivid, emotionally-toned experience. He is then read a story ("Versuchsgeschichte") so constructed by the experimenter as to be very similar in content to the experience in question. The details in the narrative, however, are incomplete, vague and only cleverly approximate the actual events. Having been instructed to pay strict attention, the subject is requested to recall as much of the story as possible. But the details in the narrative and experience being so much alike, he more or less unconsciously confuses those of one with those of the other, and, as a result, inserts and supplements items which were wholly absent from the story. By this intimate knowledge of details, he easily betrays his familiarity with certain events. He may have previously denied most emphatically the least acquaintance with a single incident. Nevertheless, his responses definitely point to the contrary.

III *The " Aussage " or Reproduction Method.* (Wertheimer.)

This procedure is not to be confused with Jung's. The individual here is asked to reproduce orally the story which had been read to him. Using the technical terms of the " Aussage " Test, this portion of the method is the " Bericht " or report, and may be further supplemented or completed by the " Verhör " (the interrogatory or deposition). The latter may be any one or a combination of these three types. First, the questions may relate to details only, which are *common* to both story and " Tatbestand " (experience, or constellation of facts); second, the questions may yield different responses owing to *differences* in story and " Tatbestand "; third, the questions may relate to details present only in the " Tatbestand." Kramer states that this method may work perfectly in the laboratory, but may fail absolutely in actual application. More data are necessary.

IV *The " Aussage "-association Method.* (Wertheimer.)

Here the subject is given an association series made up of words of these three types: first, those relating to similarities in both the " Tatbestand " and the story; second, those relating neither to the " Tatbestand " nor the story; third, those relating only to the " Tatbestand." The subject is requested to state his associations only to those words which appeared in the story; to all others he is to respond with the word, " nothing." According to Lipmann the guilty easily betray themselves.

V *The Combination Method.* (Lipmann and Wertheimer.)

The following is Lipmann and Wertheimer's adaptation of the Ebbinghaus " Kombinationsmethode." After the subject has heard the " Versuchsgeschichte," a sheet is placed before him containing the text of the story, but from which many details have been omitted. Room is left in the " Kombinationstext " for the proper information to be inserted. This information (not having been given at all in the story) either must be supplied from the "Tatbestand," or else the text is so arranged as to draw out more or less forcibly a complete description of some incident inaccurately mentioned in the narrative.[8]

Apparatus is arranged so that whenever the subject writes, an electric circuit is closed. It is opened the moment the pencil leaves the paper. A kymograph is used to record these "makes" and "breaks" of current. Long, significant pauses may thus be recorded and the correlations between length of pause and character of the supplementation noted. Attempts at dissimulation are easily discovered, first, by a too frequent response of "Nothing"; second, by too many unmeaning reactions; third, by a greatly retarded reaction procedure. At the end of the experiment, Lipmann and Wertheimer had their subject give an introspective account of his mental activity during the examination. Many of those who were found to have betrayed themselves, were most positive in their insistence that they had said nothing which could in any way reveal the existence of the complex. Rittershaus sees no value in this method.

In all of the three above procedures, it is admitted that innocent as well as guilty individuals will make errors in their reproductions. The point to be emphasized, however, is that the guilty make *significant* ones.

VI *Perception Method.* (Wertheimer and Klein.)

Optic stimuli are presented by means of a tachistoscope. The exposure is made for a very short time. The association series includes (a) purely irrelevant words, (b) complex-words, (c) irrelevant words similar by sight or sound to those of a complex type, (d) incomplete words which might be interpreted by the subject either as irrelevant or complex, e. g.,—CHO-E may be interpreted in terms of the complex as CHOKE, or may be understood by the innocent individual as CHORE or CHOSE.

Results seem to indicate that (a) so-called guilty individuals perceive complex stimuli more quickly than irrelevant, (b) irrelevant stimuli are falsely perceived as complex, (c) incomplete stimuli are interpreted more easily as complex than irrelevant.

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[8] Max Verworn suggests a graphic method whereby an individual is shown a drawing similar in detail to the place where, let us say, a crime was committed. The drawing is not quite accurate. The suspect is asked to reproduce the drawing from memory. The errors may be very significant.

Instead of perception being visual, it may be auditory. The authors suggest Gutzmann's procedure in the use of a phonograph. Thus the word *coarse* will be heard as *corpse* by the murderer. Some of the advantages in using a phonograph have already been indicated. Besides the ones mentioned, the phonograph adds obscurity and indistinctness to the stimulus, and eliminates any possibility of lip-reading.

VII *Distraction Method.* (Wertheimer and Klein.)

The subject is given two texts, the content of one being related to the "Tatbestand," while the content of the other is quite irrelevant. He is requested to put a line through all "r's" as rapidly as possible. The number of "r's" crossed, the time, errors, and behavior are noted. The results seem to show that the meaning of the text relating to the "Tatbestand" is much more distracting to the rigid and accurate performance of the task.

VIII *Sentence Method.* (Moravcsik.)

The experimental material consists largely of sentences expressive of different emotional states. Sentences of indifferent affective toning are also employed. The subject is requested to respond with whatever comes into his mind. No attempt is made to narrow down the reaction to any particular form, type or to any number of words. Moravcsik experimented only on melancholiacs and maniacs. The following are samples of his material:

Stimulus *Words* (depression)—sadness, pain, suffering, grave, sins.
(exaltation)—joy, riches, dance, laughter.
(indifferent)—garden, house, snow.
Sentences (depression)—How unfortunate I am!
It would be well if I were buried.
My heart is so full of pain.
(exaltation)—The best thing is a long life.
I am full of joy!
(indifferent)—In Winter the snow falls.

He observed that the melancholiac found a group of words expressive of dejection harmonious and desirable, and revolted to those of an exalted type. The reverse was true of the maniac. He also concludes that those possessing a stronger mental balance answer with a single word rather than with a sentence.

IX *The "Ausfrage" Method.* (Marbe, Messer, Bühler and others. Designated "Ausfrage" by Wundt.)

The procedure is that of the simple association experiment. A stimulus word is presented and the subject responds with the first idea coming to consciousness. Immediately following this, however, he is asked to give a complete introspection of all his mental operations during the reaction. This method is of value only where trained introspectionists are acting as subjects. Wundt opposed its use as a psychological experiment, since it lacks the four necessary elements, but Kakise, on the contrary, makes a strong plea for its employment. From one point of view this mode of procedure resembles greatly the usual psychanalytic method.

Since some of these methods have only in one or two cases been given a fair and accurate trial by unbiased yet expert psychometrists, for the detection of crime, it is not surprising to find so large a number of workers in this field skeptical as to their value. We certainly need more data. But, granting their worthlessness for criminal psychology, they still are of great promise to experimentalists interested in the diagnosis of individual complexes and constellations. As yet, the procedures are crude and the technique undeveloped. The methods are waiting for a master-hand at reconstruction and improvement.

G THE COMPLEX INDICATORS ("KOMPLEXMERKMALE")

In the earlier association experiments, deviations, peculiarities, errors in response were not regarded as significant. And even to-day, unless one is well acquainted with the more recent contributions in this field, one may be led to commit many farcical errors, such, for example, as those of Schnitzler. During his experiments, any stimulus word not properly understood by the subject was eliminated from the series and another similar one was substituted! This occurred in all cases at least once, and was as frequent as five or six times!! One is naturally led to conclude either that the examiner was very careless in his choice of words or in his pronunciation of them, or that his subjects were peculiarly defective in hearing, or what is more probable and evident, that he seriously neglected observing the importance of failures to hear or understand, especially as to their relation to the complex for which he was searching. On the contrary, an efficient examiner ought to look suspiciously upon every unnatural or forced laugh, cough or movement.

In this connection, it might be advisable to emphasize most strongly the fact that *one* complex-sign by itself should never be regarded as diagnostic. One may venture a judgment only when in possession of an *aggregate* of indicators. Then again, common sense must be used in determining the importance of the several symptoms. Kramer and Stern have observed that different individuals betray their complexes in different ways. Consequently the importance of the separate " Komplexmerkmale " will be differently graded for various persons. Also, A. Gross, Löffler, Kramer and Stern and Rittershaus all emphasize the danger of rendering interpretations on the basis of minutiæ. To quote Löffler, " What cannot be grasped with the hands, ought to be let alone." (" Was nicht mit Händen zu greifen ist, lasse man beiseite.")

Among the skeptics is Heilbronner, who maintains that the large number of complex-symptoms leads to inaccuracies, since to the experimenter with foresight, it is overwhelming, while for one who is careless, it means arbitrariness and caprice. He has no faith in conduct being used as a complex-sign, and believes that the existence in all persons of numerous complexes renders a separate diagnosis very difficult. Rittershaus seems to regard the last criticism as a very telling one. In actual experimentation, however, where one obtains some sort of verbal response, which may later be analyzed by a question or two, the difficulty is not as serious as it is made to appear.

The " Komplexmerkmale " may be classified roughly into three types,—qualitative, quantitive, physiological.

I *Qualitative Complex Indicators*

(1) *Content.*

A subject may often help indicate the existence of specific constellated mental material by giving a response-word of significant meaning.

(2) *Whispered Response.*

A stimulus word disturbing the usual calm of mental activity may call forth a whispered response.

(3) *Failure to Understand.*

The stimulus word is really not misunderstood, but, due to the activity of the " censor " the subject has a strong desire *not* to understand. In one case, Jung found the following to have been misunderstood: hate, love, remorse, failure, pleasant. All were related to a definite complex.

(4) *False Reproduction.* ("Wiederholung.") (Responses.)

This phenomenon has been explained earlier in the paper. The reproduction may occur after a short interval (Jung), or after a series of weekly intervals (Pfenninger). In one of Stein's cases (a woman forger), 44% of her reactions were falsely reproduced. Pfenninger has demonstrated that those stimulus words which during the first experiment, gave rise to no complex signs whatever, showed changes in reproduction (after eight intervals, a week apart), amounting to 2.6 for men, 1.6 for women. Where there was but one complex-sign (lengthened reaction time), the changes amounted to 3.3 for men, 2.4 for women. Where there were more than this one complex indicator, the changes amounted to 4.5 for men, 1.7 for women. He concludes that the greater the number of complex-signs possessed by a stimulus word, the larger will be the number of failures in reproduction; and vice versa, the greater the number of failures in reproduction, the greater will be the number of complex-signs attaching to a stimulus.

(5) *False Recall.* ("Erinnerung.") (Stimuli.)

The subject here is asked to recall the stimulus words in any order. Two types of individuals may be observed, one who tries to keep from betraying himself, and will avoid recalling very many, if any, significant words; the other type, more free and open, who will recall complex-stimuli more rapidly and in larger proportion than insignificant stimuli.

(6) *Perseveration.*

(a) Response.

Sometimes no apparent nexus is observable between stimuli and a certain kind of response which appears more or less frequently throughout the experiment. This disposition has been given the name "perseveration tendency" ("Perseverationstendenz."—Müller-Pilzecker). Perseveration may be due to the presence of a complex or a constellation which dominates consciousness, or to a marked poverty of ideas, or the cause may lie deeper in the psychophysical mechanism. Swoboda emphasizes the importance of regular periods of time upon the recurrence of ideas. One of Jung's subjects showed a perseveration of 5%. On examination, the words were all found to be related to the oppressive heat of the day. Of the repeated responses, two were "snow," and two "perspire." Perseveration may occur because of an impulse to avoid creating a "vacuum," or to break down one already existing.

(b) Stimulus.

A subject may sometimes use some significant stimulus word which appeared earlier in the experiment as the response for succeeding stimuli. Perseveration in this case may be due to any one of the reasons mentioned under the preceding heading.

(c) Affect.

Sometimes the perseveration of an affect may be noted. In such cases, the reaction times are increased. (See Post-critical Responses.) On the contrary, Schnitzler observes that affective perseveration shortens reaction times.

(7) *Inhibition.*

Some trains of thought are inhibited by the subject for reasons of modesty, privacy, fear, shame. Any response bringing these uncomfortable ideas into consciousness is checked, with a resulting increase in reaction time.

(8) *Failure to Respond.*

Aside from explaining that failures to respond may be produced by an "emotional stupidity," Jung states that the failures may be due either to hindrance of verbal expression because of the presence of vivid visual imagery, or of vivid ideas strongly tinged with emotion; or they may be due to a distinct eroticism. Sutherland seriously attacks Jung's explanation. First, he takes exception to the statement, "I call attention to the fact that it is quite indifferent what reason the test person gives for refusal to react"; and secondly, he gives the following as some of the true reasons for failures: unpreparedness, meaning recognized and inhibited, attention focussed on irrelevant matters, meaning not recognized, stimulus misunderstood, no meaning to the stimulus, in doubt of word, ambiguity of stimulus, copious imagery, fascinating visual imagery, voluntary inhibition, articulatory block, competition of reaction words, motor block, absorption in trains of imagery, revery, dreamy, blank. In summing up, he says, "A satisfactory explanation of the failure to react involves a genetic study of each particular case; that a considerable number of factors is or may be responsible for each failure; that many of these factors may be peculiar to the experimental situation. The failure indicates the previous experience of the individual only in a very general manner, and is apparently not sufficiently specific to use confidently for purposes of diagnosis." It is a bit unfortunate that Sutherland chose so many words which tended naturally to increase the normal number of failures.

In one case, Jung observed that the following words produced failures to react: caress, sick, suffer, kiss.

(9) *Quotations.*

(10) *Titles.*

(11) *Sentences.*

A stimulus may give rise to a mental state which can best express itself by means of a quotation, the title of a book, of a story, or a sentence from a song or poem. It is held by some that this is done in order to veil a secret thought. Jung explains that similarly people often sing songs and tell stories which give an intense secret pleasure, because they contain an idea, or give rise to a feeling which cannot openly be expressed.

(12) *Symbolism.*

A word may sometimes be given in response which possesses a separate, individual meaning for the subject, and acts as a symbol representative of a complex.

(13) *General Concepts Hiding the Meaning.*

What appears as an unimportant, general concept may in reality be only a cloak for some specific complex. For example, Jung cites the case of a young lady to whom he presented the stimulus, "three." The response was, "friends." The subject's later statements made it manifest that she had been painfully disillusioned by three of her intimate friends.

(14) *Addition of the Article.*

For example, "angry"—"the person." "*The* person," in this case, meant the subject's irritable father who severely objected to her relations with the young man she loved.

(15) *Naming of an Object in the Examiner's Room.*

Thinking it inadvisable to express a complex-reaction-word, an individual may begin a search for a substitute. His eyes, wandering about the room, may suddenly light upon some object, whose name he immediately proceeds to use as a surrogate-response.

(16) *Peculiar Form of the Response.*

In order to avoid a direct reply with a consequent betrayal of a complex, a person may sometimes be observed to respond with a phrase, a few incoherent or unrelated words.

(17) *Unusual Response.*

A word which is but seldom given as a response may be regarded as suspicious, and indicative of the existence of a constellation or a complex.

(18) *Errors in Response.*

Since the complex is absorbing the complete attention of the subject, all sorts of errors are liable to occur when attempting to respond to an "alien" stimulus.

(19) *Assimilation.*

Because of the subject's different apperceptive attitude, he gives a *new* and *unusual* meaning to the stimulus word.

(20) *Vacuum.*

The explanation and description of this condition have already been briefly mentioned above.

(21) *Unmeaning Responses.*

These may be of two forms, (a) the qualitative relations between a stimulus and a response are of an unmeaning or senseless character, (b) the response itself is without meaning. Rhymes and sound (Klang) associations are examples of unmeaning reactions.

(22) *Nonsense.*

Here are included those slightly irrelevant responses possessing the merest touch of humor perceptible to the examiner only.

(23) *Incomplete Responses.*

Now and then a subject may react by giving only part of a word or phrase. These "errors" should always be recorded and their relations to the complex closely scrutinized.

(24) *Supplementations of Stimulus Words.*

This activity, especially if more or less frequent, may be regarded as suspicious.

(25) *A-symmetrical Responses.*

The stimulus word, usually of double meaning, is not interpreted in its customary sense. For example, " Aussicht " means both " view " and " appearance ; " the stimulus word " mean " may be interpreted by the subject as any of the following: arithmetical average, purpose, import, vulgar, paltry, contemptible, and so on. The complex or constellation generally determines which one of these is to be perceived. Homonyms may also be included under this head,—fair, fare; crews, cruise; muscle, mussel; tare, tear.

(26) *Post-critical Responses.*

Post-critical reactions are those immediately following a critical response. Various experimenters have observed that an emotion may have a post-critical effect, tending usually to increase the reaction time. Heilbronner goes further by maintaining that not only are the next few responses affected by this perseveration, but all of the remaining experiment is absolutely changed. Only a very small number, however, regard this criticism as serious.

(27) *" Versprechen," " Verhören,"* (*" Verlesen."*)

The importance of arranging some stimuli so as to be incorrectly perceived, has already been discussed elsewhere. Examples of " Verlesen " (misread) are, (German) " Mord " for " Mond," (English) " kill " for " hill ; " " Verhören " (mishear), (German) " Molch," " Dolch " or "Strolch " for " Olch ; " (English) " die " for " buy," " truth " for " truce," " filth " for " fills," " father " for " farther," " arms " for " alms." As for " Versprechen " (misspeak), the Freudians have adequately emphasized its significance for complex-diagnosis. In any of these mechanisms the error may be wilful or unconscious. Also, the response-word may be directly related to the complex or may be the expression of a wish that the stimulus word were different.

(28) *" Komplexvertreter."* (*Complex-surrogate.*)

In such cases the subject keeps one word, usually, in reserve, in order to substitute it for complex-responses. Wells cites a case. To the words "useless," "husband," "honor," and "against," the response in each instance was "word." This was accompanied every time by a long reaction period. On the basis of other evidence, however, he denies that this is a true example of "Komplexvertreter."

(29) *Translations.*

To avoid giving a suspicious response, an individual may not infrequently translate the stimulus into a different language. Sometimes the reaction word is expressed in a foreign tongue.

(30) *Interjections or Single Letters.*

These are sometimes uttered as response words. In most cases, one is safe in regarding them as suspicious.

II. *Quantitative Complex Indicator*

The quantitative complex indicator is the reaction time. The reaction time is usually regarded as the interval between the giving of the stimulus word (the stop-watch or chronoscope being started on the accented syllable), and the pronouncing of the reaction word.

This general reaction time is composed of seven or eight subdivisions. Ziehen, Claparède and Jung have attempted to clearly distinguish these—thus:

(1) The time of transfer of the sound to the ear of the subject,

(2) the transfer from the periphery to the auditory center,

(3) the recognition of the word,—what Jung calls "primary identification,"

(4) the understanding of the word,—called by Jung, "secondary identification,"

(5) the calling forth of another idea (this is really the *pure* association),

(6) the naming of this idea in consciousness,

(7) the inner stimulation of the verbo-motor apparatus (and if a key is to be pressed, also a stimulation of the motor centers controlling hand movements),

(8) the time between this stimulation and expression.

The gross reaction time has been found to be:

Aschaffenburg's figures	1.2 to 1.4 seconds
Galton's figures	1.3 seconds
Trautscholdt's figures	0.9 to 1.2 seconds
Féré's figures	1.6 for men, 1.8 for women
Jung's (already mentioned)	1.8 average.

According to Jung, a 0.5-second variation over the average may be regarded as suspicious. That is probably too short an allowance,—one second is probably better.

It is generally agreed that the stop-watch is, from many points of view, the better instrument with which to measure association time.

III. *Physiological Complex Indicators*

The physiological complex indicators will merely be mentioned, since, as yet, we know but little of their value as practical instruments for the detection of complexes. It is at present assumed that unusual changes produced in any of the following, by the perception of a stimulus word, may be diagnostic of a complex:

(1) *Psychogalvanic Reflex.*
(2) *Electro-motor Heart Flow.*
(3) *Pulse.*
(4) *Respiration.*
(5) *Blood Pressure.*
(6) *Involuntary Movements, or Tremors of the Hands or the Limbs.*
(7) *Ataxiagraph. (Involuntary Bodily Sway.)*
(8) *Knee-jerk.*
(9) *Lifting Ability of the Finger. (Ergograph.)*
(10) *Tapping Test.*
(11) *Strength of Grip.*
(12) *Facial Expression. (Reddening, etc.)*
(13) *Tone of Voice in Response.*
(14) *Ability to Concentrate. (Disturbances of Attention.)*
(15) *General Conduct and Behavior. (Coughing, Clearing the Throat, etc.)*

* * * * * *

It is generally admitted that a large number of the complex-indicators mentioned above may be purely accidental and without any complex-significance whatever. For example, a person with a cold in the head may not hear very well, he may give a wrong response, may make some unusual, restless movements, may cough here and there, and tears even may begin to flow from his eyes. The experimenter cannot be too careful in discriminating a forced, artificial behavior from a purely natural one. Unless one is heedful of this fact, most absurd results may be obtained.

The complex-indicators have not been arranged in order of importance, for as previously mentioned, each individual has an order of his own. Rittershaus emphasizes the fact that since uneducated possess a much smaller vocabulary, they may give longer reaction-times when presented with slightly strange words; that, coming from a lower level, they are not accustomed to think in single words or ideas, but in sentences, and that they often approach the examination as a kind of intelligence test, thinking that they must under no circumstances give anything but an intelligent answer. In such cases, it is not surprising to observe failures to respond, lengthening of reaction times, false reproductions, sentence responses, etc. These complex-indicators are of practically no significance in their cases.

The experimenter should try to observe as many symptoms as is possible without interfering in any way with the mental composure of the subject. For that reason, too much apparatus in the detection of physiological complex-indicators is not advisable.

Rittershaus' experimental procedure might be mentioned here. There are four experimenters: the first gives the association series and records the reaction word and the time; the second has a copy of association material and sits a little on the side, paying particular attention to the behavior of the subject, noting any physiological changes or changes in behavior; the third is a disinterested individual who is not present during the examination. After it is over, he takes the results and works them into curve form; and the fourth is one who knows nothing of the particular subject tested, and is ignorant of all direct experimental data. He studies the curve and draws his conclusions from it regarding the existence of complexes. When that is done, all get together and talk it over.

Before the experimenter can feel properly qualified to evaluate results, he must have had a wide experience in applying this method to different types of normal adults, to children, to feeble-minded, and to insane. To quote Jung: "The association method is a tender instrument which can only be directed by the experienced hand, and one must pay dearly for his experience before the application of this method is accurately understood. Consequently, as it now stands, one must not expect too much; it is, however, full of great possibilities."

H. CONCLUSIONS

I. *Pedagogical*

Ziehen places strong emphasis upon the fact that experimental studies of association are necessary for a scientific pedagogy. This matter has been reiterated by Meumann.

Pfister clearly indicates the relation of the complex to the hygiene of the school child. A number of psychanalysts are now studying the complex as a moulder of character, and as a determiner of the future.

From the data already considered, we may infer the following:

(1) Every one of us possesses a different mass of associated complexes. In the school room, consequently, an explanation to one will have an entirely different meaning to another pupil.

(2) Education fixes certain associations between ideas. These associations are not easily broken down. Sometimes they persist for the whole life-time of an individual. What great care and caution must be exercised by the teacher!

(3) Galton has shown that the largest part of our associations are " histrionic," of the motor or visual type. For that reason, it should be emphasized that the greater the amount of objective experiences obtained by the child, the better equipped will he be later. These experiences should be not only perceptive, but also *affective*. Ziehen and Meumann found that children between six and thirteen who gave a larger per cent. of " individual vorstellungen " (particular ideas), were the brighter. These particular ideas naturally give the child a sound basis on which to build his abstractions.

(4) Galton also pointed out that nearly 50% of our oft-repeated associations date back to childhood and youth. How important is it then, to fill the mind of that period with rich, numerous, and concrete associations.

(5) The emotions greatly affect the facility of associations. Pleasant emotions help, unpleasant ones retard, the process. Consequently, our aim in education should be for pleasurable feelings all the time, in order to facilitate the acquisition of knowledge. " Happiness first,—all else follows," to quote the Vineland motto.

(6) Emma Fürst has shown how the association complexes of the family have an enormous directing effect upon the formation of complexes in the children. The family constellations (what Jung calls the " infantile milieu ") have an enormous effect upon the character formation of the child.

Adler has clearly emphasized the importance of the " manly protest " at the dawn of puberty. At that period a most significant revolution, both mental and physical, is going on in the individual. The spirit and desire for freedom, for self-expression, are raging in the entire organism. Consequently, a child who is bound down by, and too much dependent upon the family and its members, may suffer serious mental injury.

(7) Children's questions ought to be given more earnest consideration. Our treating their every inquiry as a matter for mirth is absolutely unjust. Truth and seriousness ought to prevail instead.

(8) If the Freudian concepts of infantile sexuality are true, then we may hopefully look to psychanalysis not only as the most essential neurotic prophylactic, but also as a most important factor in determining the directions in which personality is expected to develop. As regards the first, Pfister already has indicated how certain abnormalities in school children may be psychanalyzed and removed.

II. *Psychological*

If the psychology of character has a future then the search for complexes is going to play an enormous part in its development. At present, we have not advanced very far beyond the classification of the temperaments. In Wundt's psychology, the four-fold arrangement of the Greeks still persists. Other classifications have been suggested by Dessoir, Bahnsen, Höffding, Ebbinghaus, Ribot, Kraepelin, Binet and a large number of others, but they are all purely theoretical. And one is painfully astonished to note in a recent German article on personality, but one(!) reference to the work of a psychanalyst among 130 others. "Komplexlehre" was entirely neglected in this study.

The usual definition of character focusses on the term "expression," but a new definition, in the light of complex and constellation studies, will go back to the *motives* for this "expression." The one is objective, the other subjective.

Sutherland has indicated that trains of association are very significant for (1) information regarding the condition of mentality—normal or abnormal; (2) range of information in any particular subject; (3) logical sequence of the thought processes.

Further research in this extensive field of the complex will no doubt reveal the closer union between psychology and eugenics. Are the tendencies to acquire certain particular complexes hereditary? Are complexes, as such, hereditary? The studies of Emma Fürst seem, on the surface, to give a positive answer to at least the first question.

In closing this paper, I cannot but emphasize the importance of the complex and the constellation as factors shaping our destinies, and the importance of the association method in determining what these complexes and constellations are.

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K. APPENDIX

A TABULAR LIST OF COMPLEX INDICATORS

I. *Qualitative*

(1) Content
(2) Whispered Response
(3) Failure to Understand
(4) False Reproduction
(5) False Recall
(6) Perseveration
    (a) Response
    (b) Stimulus
    (c) Affect
(7) Inhibition
(8) Failure to Respond
(9) Quotations
(10) Titles
(11) Sentences
(12) Symbolism
(13) General Concepts Hiding the Meaning
(14) Addition of the Article
(15) Naming of an Object in the Examiner's Room
(16) Peculiar Form of the Response
(17) Unusual Response
(18) Errors in Response
(19) Assimilation
(20) Vacuum
(21) Unmeaning Responses
(22) Nonsense
(23) Incomplete Responses
(24) Supplementations of Stimulus Words
(25) A-symmetrical Responses
(26) Post-critical Responses
(27) "Versprechen," "Verhören," ("Verlesen")
(28) "Komplexvertreter" (Complex-surrogate)
(29) Translations
(30) Interjections or Single Letters

II. *Quantitative*

(1) Time

III. *Physiological*

(1) Psychogalvanic Reflex
(2) Electro-motor Heart Flow
(3) Pulse
(4) Respiration
(5) Blood Pressure
(6) Involuntary Movements, or Tremors of the Hands or the Limbs
(7) Ataxiagraph (Involuntary Bodily Sway)
(8) Knee-jerk
(9) Lifting Ability of the Finger (Ergograph)
(10 Tapping Test
(11) Strength of Grip
(12) Facial Expression (Reddening, etc.)
(13) Tone of Voice in Response
(14) Ability to Concentrate (Disturbances of Attention)
(15) General Conduct and Behavior (Coughing, Clearing the Throat, etc.)

Kent-Rosanoff Association Series

1. table
2. dark
3. music
4. sickness
5. man
6. deep
7. soft
8. eating
9. mountain
10. house
11. block
12. mutton
13. comfort
14. hand
15. short
16. fruit
17. butterfly
18. smooth
19. command
20. chair
21. sweet
22. whistle
23. woman
24. cold
25. slow
26. wish
27. river
28. white
29. beautiful
30. window
31. rough
32. citizen
33. foot
34. spider
35. needle
36. red
37. sleep
38. anger
39. carpet
40. girl
41. high
42. working
43. sour
44. earth
45. trouble
46. soldier
47. cabbage
48. hard
49. eagle
50. stomach
51. stem
52. lamp
53. dream
54. yellow
55. bread
56. justice
57. boy
58. light
59. health
60. bible
61. memory
62. sheep
63. bath
64. cottage
65. swift
66 blue
67. hungry
68. priest
69. ocean
70. head
71. stove
72. long
73. religion
74. whiskey
75. child
76. bitter
77. hammer
78. thirsty
79. city
80. square
81. butter
82. doctor
83. loud
84. thief
85. lion
86. joy
87. bed
88. heavy
89. tobacco
90. baby
91. moon
92. scissors
93. quiet
94. green
95. salt
96. street
97. king
98. cheese
99. blossom
100. afraid

Jung Association Series

1. head
2. green
3. water
4. to sing
5. dead
6. long
7. ship
8. to pay
9. window
10. friendly
11. to cook
12. to ask
13. cold
14. stem
15. to dance
16. village
17. lake
18. sick
19. pride
20. to cook
21. ink
22. angry
23. needle
24. to swim
25. voyage
26. blue
27. lamp
28. to sin
29. bread
30. rich
31. tree
32. to prick
33. pity
34. yellow
35. mountain
36. to die
37. salt
38. new
39. custom
40. to pray
41. money
42. foolish
43. pamphlet
44. despise
45. finger
46. expensive.
47. bird
48. to fall
49. book
50. unjust
51. frog
52. to part
53. hunger
54. white
55. child
56. to take care
57. lead pencil
58. sad
59. plum
60. to marry
61. house
62. dear
63. glass
64. to quarrel
65. fur
66. big
67. carrot
68. to paint
69. part
70. old
71. flower
72. to beat
73. box
74. wild
75. family
76. to wash
77. cow
78. friend
79. luck
80. lie
81. deportment
82. narrow
83. brother
84. to fear
85. stork
86. false
87. anxiety
88. to kiss
89. bride
90. pure
91. door
92. to choose
93. hay
94. contented
95. ridicule
96. to sleep
97. month
98. nice
99. woman
100. to abuse

Jung's Association Series as Modified by White

1. head
2. green
3. water
4. to prick
5. angel
6. long
7. ship
8. to plough
9. wool
10. friendly
11. table
12. to carry
13. insolent
14. to dance
15. lake
16. sick
17. proud
18. to boil
19. ink
20. angry
21. needle
22. to swim
23. journey
24. blue
25. bread
26. to threaten
27. rich
28. lamp
29. tree
30. to sing
31. sympathy
32. yellow
33. mountain
34. to play
35. sail
36. new
37. custom
38. to ride
39. wall
40. stupid
41. volume
42. to despise
43. teeth
44. correct
45. crowd
46. book
47. unjust
48. frog
49. to cut
50. hunger
51. white
52. ring
53. to listen
54. pencil
55. woods
56. apple
57. to meet
58. low
59. love
60. glass
61. to quarrel
62. goat
63. large
64. potato
65. to paint
66. part
67. old
68. flower
69. to strike
70. box
71. wild
72. bright
73. family
74. to wash
75. cow
76. stranger
77. luck
78. to tell
79. hesitation
80. narrow
81. brother
82. to harm
83. stork
84. false
85. anxiety
86. to kiss
87. fire
88. dirty
89. door
90. to choose
91. hay
92. quiet
93. scorn
94. to sleep
95. month
96. colored
97. dog
98. to talk
99. carriage
100. sky
101. straw
102. baby
103. to lie
104. blood
105. duty
106. bed
107. to rent
108. sorrow
109. mirror
110. prison
111. knee
112. to live
113. change
114. barn
115. snake
116. to uncover
117. policeman
118. wagon
119. judge
120. night

Other words may be interspersed if significant.

BOOK NOTES

Canestrini, Silvio. Ueber das Sinnesleben des Neugeborenen. Monographien aus dem Gesamtgebiete der Neurologic und Psychiatrie. Herausgegeben von A. Alzheimer, Breslau and Lawandowsky, Berlin. Berlin, Springer, 1913. 104 p.

The author of this monograph has attempted to study the sense development and reactions of the newborn child in waking and sleeping condition by a more exact method than has usually hitherto been used. The method employed consisted in testing the cerebral reactions of the child to various sensory stimuli by adjusting a specially devised apparatus, a modification of the Marey pneumograph, to the fontanelle of the child thus testing the cerebral pulse directly with simultaneous records of breathing. For the subjects of his experiments he chose infants of the age of one to fourteen days, selecting only those who had large and pulsating fontanelles so that the brain pulse and variations in the volume of the brain would be sufficiently pronounced to give graphic results. Altogether about 70 infants were studied and over 700 separate experiments were made. Different stimuli for the five different senses were employed, and the tests were made both in sleeping and waking condition, and the emotional condition of the child studied also.

The results of Canestrini's experiments in part corroborate and in part modify the results found by Kussmaul, Genzmer, Preyer and others who have studied the development of infants. Only in the sense of taste does the newborn child show a strikingly complete development and stands only a little below adult animals. As regards smell the human infant is far below newborn dogs and cats. The difference between the infant and the animal is still more striking when we consider the function of motility in general and certain psychic performances.

Among the most important results of Canestrini's own experiments are the following: The special tests of reaction to various sensory stimuli by the fontanelle method show that in the infant the sense of taste gives the most developed reactions, next perhaps are the reactions of the organ of hearing, no child being found among the 70 tested who did not react to acoustic impressions. From the day of birth on, light stimuli of definite intensity clearly influenced the breathing and the brain curve. The sense of touch showed strikingly slight reaction to pain stimuli and little excitability of the infants' nervous system to the electrical current. The strongest and promptest reaction occurred for stimuli of cold, which caused acceleration of breathing, increase of the brain volume, motor disturbances, but no regular change of the pulse frequency. The sense of smell showed the least development, as suggested by the author probably because an atrophy of this sense has already occurred.

This objective method which has been used also by Bechterew promises to give important results in case of children whose skulls are sufficiently metopic to allow the successful use of it.

W. H. B.

Psychologie der frühen Kindheit. Von WILLIAM STERN. Leipzig, 1914. 360 p.

Educators and psychogeneticists have long wanted a psychology of the early years of infancy and childhood. Here we have, so far as we know, the first attempt to furnish such a psychology, which follows certain main lines of interest from birth to the age of six years. The author begins with very general considerations about methods of child psychology, psychic developments, convergence, etc. He then takes up the speechless age, discusses its development of motive, the beginning of play organization, conquest of space memory, the activity of the Gemüth, etc. In the next part he discusses the development of speech, its four stages and beginnings, the end and composition of child language and its content, the factors and chief epochs. He then discusses the contemplation of pictures; then in successive sections, memory, fancy, play, forms of childish thought, the life of the soul and Gemüth, fear, aesthetic sensibility, feeling and treatment toward others, the effect of punishment, etc.

Probleme der Mystik und ihrer Symbolik. Von HERBERT SILBERER. Wien und Leipzig, Hugo Heller & Co., 1914. 275 p.

After an introductory part treating of dreams and interpretation of Märchen, comes second, an analytic part treating the psychoanalytic meaning of the parabola, alchemy, hermetic art, crossing of races and free masonry, and the problem of manifold interpretation. The third, synthetic part deals with introversion and rebirth, first sensationalism and introversion, second, the consequences of the latter, and third, regeneration. Finally the writer discusses the mystic goal and the royal art. There is a copious literature and a convenient index.

The Great Society. A Psychological Analysis. By GRAHAM WALLAS. New York, Macmillan Co., 1914. 369 p.

This work treats of social psychology, instinct, intelligence, philosophy and environment, fear, pleasure, pain, happiness, the psychology of the crowd, love and hate, thought, the organization of thought and will, and the philosophy of absence.

Die Mystikerin Margaretha Ebner (c. 1291-1351). Von Dr. LUDWIG ZOEPF. Verlag B. G. Teubner, Leipzig und Berlin, 1914. 177. p.

This is a very interesting study of a mediaeval mystic and of her time. She was born in 1291 and her mystic period began in 1335, when she had a very vivid picture of the sufferings of Jesus and became mystically united with him. Then follows an account of her various dreams and visions, and finally her relations to her own period, to the cloister, the outer world, etc. The work is done with great thoroughness and there is a long list of authors.

The Elements of Psychology. By DAVID R. MAJOR. R. G. Adams & Co., Columbus, Ohio, 1914. 403 p.

In the present revision of this text two years after it first appeared we are told that "many paragraphs and sections have been rewritten. A number of new paragraphs have been added, two new figures have been introduced, and the topics in several of the chapters have been rearranged."

The Vital Law. By Edwin M. Johnson. Vital Law Publishing Company, Kansas City & London, 1914.

Vol. 1. The Force That Operates Every Organism. 301 p.
Vol. 2. How Mind Causes the Force of Organism to Heal or Destroy. 302 p.
Vol. 3. How Mind Causes Chemical, Physical and Vital Changes. 291 p.

These three well printed volumes deal with such topics as the following: the force that works from within; the element of force; life, force and the body; hearing and seeing vital truths; life to them in the shadow of death; mind causes vital physical changes; the tongue heard and answered; suggestive therapeutics and the vital law; thy faith; physical, chemical and vital laws; the languages we interpret and speak; physiology; sundry links of evidence; scriptural guides; the vital law. This work is designed to be practical rather than theoretical, intuitive rather than erudite, insightful rather than scientific. One would hardly expect in a work of this kind that the author would be aware of the latest results of science where they touch his field; yet on such topics as hypnotism there are formal quotation from various modern authors; on the cell, quotation from E. V. Wilson; Burke, Origin of Life; on the senses Herter is quoted, and many other writers including scripture.

Psychology. General and Applied. By Hugo Münsterberg. New York & London. D. Appleton & Co., 1914. 470 p.

This seems to be the twenty-first volume of this prolific writer. One of his first books was on psychology and life, in which the argument was that they had no connection. This is designed to show almost the exact opposite. He says that the plan and aim of this book is very different from all the bewildering variety of other textbooks in that it includes applied as well as general psychology. "Hitherto textbooks have been confined to theoretical study. The time seems ripe for bringing the psychologist's work into full contact with the practical efforts of civilization." Moreover, we are told that this book is unique in that it includes social psychology. In the introduction he discusses aims, then casual psychology, its principles, and the elementary, group and social processes.

Then on page 285 begins what he calls purposive psychology, viz., individual and social experiences. Book three, beginning page 331, is called applied psychology, psycho-technical science, etc. Our own impression is that it is this section that is weakest and particularly the medical psychology which the author, who is believed to be a physician, so ignores.

The Psychological Monographs. Edited by Carl E. Seashore. Psychological Review Co., Princeton, N. J. & Lancaster, Pa., June, 1914. University of Iowa Studies in Psychology, No. VI. 177 p.

This is a study in accuracy of the voice in simple pitch singing by Walter R. Miles, the effect of training in pitch discrimination by F. O. Smith, the lower limit of tonality by T. F. Vance, who also has a paper on variation in pitch discrimination within the tonal range, and finally there is a study by R. M. Stewart on the effect of intensity and order on the apparent pitch of tones in the middle range, and a paper by Clara Hancock on the effect of intensity of sound upon the pitch of the low tones.

Die Philosophie der Gegenwart. Herausgegeben von Dr. ARNOLD RUGE. Heidelberg, Weiss'sche Universitätsbuchhandlung, 1914. 324 p.

This fourth volume contains titles from 1, The American Journal of Psychology, to 3531, Ziese, Wachstum und Entwicklung, and is well indexed.

Berlin in Wissenschaft und Kunst. Ein Akademisches Auskunftsbuch nebst Angaben über Akademische Berufe. Von Prof. Dr. WILHELM PASZKOWSKI. Berlin, Weidmannsche Buchhandlung, 1910. 336 p.

Die wirklichen Tatsachen der reinen Erfahrung, eine Kritik der Zeit. Von FRANZ SELETY. Leipzig, Johann Ambrosius Barth, 1913. 15 p. Sonder-Abdruck aus der Zeitschrift für Philosophie und philosophische Kritik.

Fortschritte der Psychologie und ihrer Anwendungen. Herausgegeben von Dr. KARL MARBE. III. Band—I. Heft. Leipzig, B. G. Teubner, April, 1914. 42 p. (Zur Psychologie des Denkens.)

Fortschritte der Psychologie und ihrer Anwendungen. Herausgegeben von Dr. KARL MARBE. II. Band—VI. Heft. Leipzig, B. G. Teubner, April, 1914. p. 322-371.

The Thirteenth Yearbook of the National Society for the Study of Education. Part II, Plans for organizing school surveys with a summary of typical school surveys. By H. L. SMITH & CHARLES H. JUDD. Chicago, University of Chicago Press, 1914. 85 p.

Wort und Seele: eine Untersuchung über die Gesetze in der Dichtung. Von HELLMUTH FALKENFELD. Leipzig, Felix Meiner Verlag, 1914. 132 p.

A BIBLIOGRAPHY OF THE SCIENTIFIC WRITINGS OF WILHELM WUNDT

By E. B. Titchener and W. S. Foster

(*Sixth Supplementary List*)

1901

(6) *Lectures on Human and Animal Psychology,* translated by J. E. Creighton and E. B. Titchener. Third edition. Large 8vo. London, Swan Sonnenschein & Co.; New York, The Macmillan Co. pp. x., 459.

1913

(5) *Einführung in die Psychologie.* Neue Auflage. 8vo. Leipzig, R. Voigtländer. pp. viii., 129. [See 1911 (9).]

1914

(1) *Zur Frage der umkehrbaren perspektivischen Täuschungen.* Psychologischt Studien, IX., Heft 3 und 4, Jan., 272-277.

(2) *Völkerpsychologie.* Eine Untersuchung der Entwicklungsgesetze von Sprache, Mythus und Sitte. Vol. v. Mythus und Religion, zweiter Teil. Zweite, neu bearbeitete Auflage. Large 8vo. Leipzig, A. Kröner. pp. xiii., 494. [See 1910 (6).]

(3) *Einleitung in die Philosophie.* Sechste Auflage. Mit einem Anhang. etc. Large 8vo. Leipzig, A. Kröner. pp. xviii., 448.

(4) *Sinnliche und übersinnliche Welt.* Large 8vo. Leipzig, A. Kröner. pp. viii., 423.

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